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THE
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ON DIETETIC CHEMISTRY.

There is no part of chemistry which abounds in more extraordinary disclosures than that connected with the composition and properties of *organic products*, and it is especially interesting when considered in reference to the food of man and of other animals; the recent progress of vegetable and animal chemistry has thrown some new light upon this subject, and though still enveloped in mystery and difficulty, it offers so many points of popular interest, that an outline of it will not be misplaced in this Journal. We shall endeavour, therefore, to bring together some of the most striking facts connected with it, by pointing out the chemical properties and uses of the several vegetable and animal substances constituting our ordinary articles of diet; showing how they are analyzed, how formed, how their adulterations and impurities are detected, and, above all, directing attention to the extraordinary manner in which the organic and inorganic kingdoms of nature mutually and reciprocally pass into each other, in such a way that elements which have been pent up for countless ages in the mineral world, shall, in due season, be transferred to the organic creation, and after having there for a time played their parts as portions of living beings, are again consigned to inorganic inactivity.

In reference to *dietetic* chemistry, our illustrations must chiefly be derived from and apply to such organic products as constitute our ordinary food; and this, in regard to omnivorous man, is a wide range; our teeth, and our organs of digestion, are so constructed as to show that we are intended to live upon a mixture of animal and of vegetable food; and accordingly, vegetable and animal substances are also so constituted, as to administer to the necessities of our nature; the consideration of the former part of this subject belongs to the anatomist and to the physiologist; but the qualities of the latter are objects of chemical research, and the inquiry into their nature and properties leads to some very extraordinary inferences, and opens to us a book "wherein men may read strange matters."

In the first place it teaches us, that, infinitely diversified as organic products apparently are, none of them seem *essentially* to contain or include more than three or four elementary substances. Fifty-four elements are presented to us by nature; of these, only four are employed in the elaboration of all the wonderful creations of living matter. Flowers and perfumes, leaves and wood, food and poisons—flesh, bone, fat, hair, feathers—when the chemist comes to look at all these, he finds that such wonderful diversity is the result, not of the employment of a multiplicity of elements, but of the combination of a very few, in varied *proportions*, and under the influence of *vital agency;* for, having resolved organic matter into its ultimate elements, we in vain endeavour so to recombine them as to form what we set out with. We can resolve wheat-flour, for instance, into carbon, oxygen, nitrogen, and hydrogen; but we in vain attempt to reproduce wheat-flour, or any thing remotely approaching to it, by any possible artificial re-union of these four elements; and accordingly it has sometimes been supposed that some error must of necessity contaminate our reasoning upon these matters: but the truth is, that the cause of our failure apparently lies in our utter inability so to present these elements to each other as they doubtless are presented to each other in the organs and vessels of living beings. Although, therefore, we can prove by irrefragable evidence, that the elements of sugar are carbon and water, we certainly cannot re-make sugar by any purely artificial combination of those bodies; and this, in all probability, for no other reason than that we want the command of the circumstances under which the carbon and water, or, in other words, the carbon, the hydrogen, and the oxygen, are presented to each other in the miraculous organization of the plant. Yet is nothing easier than to show that by feeding a plant with carbon, and with water, or its elements, these will so combine within its vessels, as actually to constitute sugar*. In fact, it all amounts to this; that the same elements which in *organic* bodies are united in *ternary* and *quaternary* combinations (that is, three or four together), have, in *inorganic* bodies, a singular tendency to combine in *binary* proportions (that is, two and two together). For instance, *carbonate of ammonia* is an artificial product, and *gelatin* is a natural or *organic* product; the *ultimate elements* are the same in both, namely, carbon, hydrogen, oxygen, and nitrogen: in the artificial compound the carbon and oxygen are so united as to form *carbonic acid*, and the nitrogen and hydrogen constitute *ammonia;* so that carbonic acid and ammonia are binary compounds, and they are the proximate components of *carbonate of ammonia*, which is also a binary compound. But in *gelatin* (or common glue), which is exclusively an *organic* product, the same ultimate elements are united into one quaternary whole; and if I decompose glue, I can resolve it into carbonate of ammonia: but I cannot *therefore* convert carbonate of ammonia into glue, because I cannot subject carbon, hydrogen, nitrogen, and oxygen (its ultimate elements) to each other's action, under circumstances which shall in any way correspond with those in which gelatin was formed.

In considering the peculiarities of organized products, there is another circumstance which must be taken into the account, namely, that even in the living body, *products which have once been organized*, appear to be requisite for the formation of new products of organization. Thus, vegetable life is in great measure supported by the decay of former vegetables; graminivorous animals require *vegetables*, and carnivorous animals *flesh*, for their subsistence. In reference, therefore, to ourselves, we also require the products of vegetable and animal organization for our support; and the inorganic elements, or their binary compounds, are utterly inefficient as *nutriment:* we should, for instance, soon starve upon charcoal and water, though we might subsist upon sugar, and analogous ternary products. If we now look more closely into the details upon which these statements are founded, we shall find that the *vegetable kingdom* is the laboratory, as it were, in which the elements of inorganic nature are so combined as to be fit for animal food; for a vegetable can live upon carbon, oxygen, and hydrogen, or at least upon carbonic acid and water, although some portion of organic matter is required for the growth of a *seed*, and although, during the process of germination, it feeds as it were upon itself; yet, as soon as the fibrils of its rootlet are perfected, and its leaves spread out to the sun and air, it absorbs water and carbon, and elaborates them into gum, sugar, starch, and other products; such, for instance, as we find in the *grasses* that cover the earth. With these the bounty of Nature has clothed the surface, and has so constituted them as to be fit for the food of graminivorous animals; in their organs they are so modified, as to become parts of themselves; and thus, the vegetable elements are transferred to the animal kingdom, through the medium of the graminivorous tribes; and through them, to man himself.

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* A very striking illustration of the composition of sugar is afforded in its sudden decomposition by sulphuric acid, which resolves it into carbon and water; for this purpose, about a table spoonful of powdered sugar may be mixed into thin paste with a little water, and rather more than its bulk of sulphuric acid added; on stirring them together, the sugar presently blackens, and then froths up into a dense carbonaceous magma, exceedingly bulky, and shooting like a cauliflower out of the glass in which the experiment is made. During the separation of the *charcoal*, a large quantity of *steam* is also evolved.

Looking, then, at the subject in this light,—that is, considering the transfer of matter from one kingdom of nature into another; tracing the atoms from the inorganic creation, from the earth, the air, and the ocean, through the vegetable world into the animal frame, and thence, back again, from the death and decay of animals, through the organs of plants, and from them to inanimate nature, what a wonderful picture is presented of mutability on the one hand, and of indestructibility and duration on the other: the organic forms are continually in a state of production and decay; the ultimate elements, permanent and immutable! For chemistry teaches us, that what is vulgarly called decay or corruption, is merely the disposition or arrangement of the same materials, the same ingredient elements, into other forms; not a single atom is ever lost or destroyed, nor is any element transmuted or annihilated. Examine, for instance, one of the most perfect cases of apparent destruction or annihilation—that presented by *fire;* a taper burns away, and gradually seems to vanish, and we naturally enough suppose that it is annihilated and lost. But when the question is more exactly investigated, what do we find actually to have happened? We trace, in the invisible stream of air which ascends from the glowing flame, all the materials or *ultimate elements* of the wax passing off into, and dissolved by, the air, and wafted away into the remote regions of the atmosphere; the *matter*, however, is not *destroyed*, but so far from it, has only become what it was before it existed in wax, an active agent, that is, in the business of the world; a main support of vegetable life; and still susceptible of running again and again the same round, as circumstances may determine; again absorbed by plants, and again becoming a part of the frame of a living being*.

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* See Herschel's *Preliminary* Discourse.

Having now said enough upon this prolific subject, to remove any doubts which might *à priori* be entertained respecting the correctness of the statements which we shall make, and the legality of the inferences which we may draw, in reference to the transfer of elements from one kingdom of nature to another, and to the infinite diversity of characters under which they appear, it may be right to give a few general notions of some of the properties of the substances which have been represented as the *ultimate elements* of the different combinations which contribute to our sustenance, taking them in their pure and simple form, and looking at such of their qualities as enable us to recognise them when masked in organic characters.

The element which preponderates in the organic creation is *carbon*, but how or in what state it exists in vegetable and animal products, it is difficult to imagine, when we compare their properties with those of pure carbon. Perhaps the *diamond* is the only known form of *pure* carbon; charcoal, lampblack, and plumbago, which are modifications of carbon, are nearly, but not absolutely pure; however carefully we prepare charcoal of burned wood, it retains traces of alkalies and earths, or of their bases; lampblack contains hydrogen; and plumbago yields indications of titanium, iron, and silicium. All the forms of carbon are distinguished by absolute infusibility, and by their extreme fixity at all known temperatures; for, provided air be carefully excluded, pure carbon shows not the slightest tendency to liquefy or evaporate at any known temperature. Yet, in the process by which steel is manufactured, we gain indications of the permeation of the iron apparently by the *vapour* of carbon; and if dust or fragments of diamond be included in a hollow wire, or tube of pure iron, heated to whiteness by voltaic electricity, the diamond disappears, and the iron becomes steel. But all direct attempts either to fuse or to vaporize carbon, have failed. The powers of combination possessed by carbon are, however, considerable, and (in reference to our present subject,) it may be combined with oxygen, hydrogen, and nitrogen, the elements with which it is associated in organic bodies; but then, these artificial compounds all bear the stamp of their origin, and are quite distinct from those which nature elaborates in organized structures: *they are all binary compounds;* carbonic oxide and carbonic acid are produced by the union of carbon with oxygen; the various hydrocarbons, by its union with hydrogen; and cyanogen by its union with nitrogen; and even here we are obliged to resort to organic sources for the hydrocarbons and for cyanic compounds; and although we have certain artificial products in which the above elements are in ternary or quaternary combination, yet they are quite unlike the organic compounds, and are generally, moreover, of organic origin; such as carbonate of ammonia, hydrocyanic acid, and some cyanurets.

The result of the imperfect combustion of carbon in oxygen, is *carbonic oxide;* and *carbonic acid* is produced by its perfect combustion in excess of oxygen, or by the unlimited access of atmospheric air aided by sufficient temperature; and, inasmuch as carbonic acid is easily formed, easily recognised, and easily separated from other gases, we have recourse to its formation when we wish to ascertain the *quantity* of carbon contained in any organic product. It is formed, for instance, by burning any substance containing carbon, in excess of oxygen; it is recognised by rendering lime-water turbid, and by occasioning in it a white precipitate of carbonate of lime; and it may be separated from the other gases with which it is usually mixed when formed as above, by the action of a solution of caustic potash, which absorbs it. 100 cubical inches of carbonic acid weigh about 47 grains; and every 22 grains of carbonic acid contain 6 grains of carbon; so that, upon these *data*, the quantity of carbon contained in any given volume of carbonic acid, produced by the combustion of any given weight of an organic product, may be determined.

Adverting to the universal existence of carbon in organic matter, Dr. Prout observes*, that in accommodating to such an extraordinary variety of changes, a material so unpromising and so refractory as charcoal, and in finally uniting it with the human mind, the Deity has, perhaps, displayed a greater stretch of power, than was requisite for the creation of the human mind itself. But to Him all things are alike easy of accomplishment, and He doubtless has willed these, and other proofs of his omnipotence, in order to convince us of this truth, that the Creator of the mind could alone have created the matter with which the mind is associated.

The proportion of *hydrogen* in organic products is determined by carefully collecting the *water* which is produced along with the carbonic acid, during their combustion in oxygen; for water is a binary compound of oxygen and hydrogen, consisting of 8 parts of oxygen and 1 of hydrogen; so that every 9 parts by weight of water produced during combustion, indicate the existence of 1 part of hydrogen.

The presence of *oxygen* in organic products is shown by their yielding carbonic acid, or water, or both, when heated in close vessels, free from air, or any other extraneous source of oxygen; to determine its quantity, a given weight of the organic matter is burned in excess of oxygen, and if no nitrogen be present, the weight of the carbon and hydrogen are determined, and the deficiency in their united weights, as compared with the original weight of the substance analyzed, is equal to that of the oxygen. Suppose, for instance, that in burning 15 grains of *sugar* in excess of oxygen, we obtain carbonic acid equal to 6 grains of carbon, and water equal to 1 grain of hydrogen, we conclude that the deficiency of 8 grains is oxygen. So that 15 grains of sugar consist of

$$\begin{array}{lr} & \text{Grains.} \\ \text{Carbon . .} & 1 \\ \begin{array}{l}\text{Hydrogen .}\\ \text{Oxygen . .}\end{array} & \left.\begin{array}{r}6\\ 8\end{array}\right\} = 9\ \text{water.} \\ & \overline{15} \end{array}$$

But the proportion which the hydrogen here bears to the oxygen, is precisely the same as that in *water*: (water being composed of 1 part by weight of hydrogen, and 8 parts of oxygen.) So that we thus come to the extraordinary, but just conclusion, respecting the *ultimate* composition of sugar, which has been above alluded to, and which represents it as a *hydrate of carbon;* that is, as composed of water and carbon.

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* *Bridgewater Treatise.*

When *nitrogen* is present in any organic product subjected to the preceding method of analysis, it escapes in the gaseous state, during combustion, along with the carbonic acid gas; the latter is abstracted by a solution of caustic potash, and the nitrogen remains, every 100 cubical inches weighing about 30 grains.

Such is an outline of those methods of analysis by which chemists ascertain the relative proportions of the carbon, hydrogen, oxygen, and nitrogen, which are the ultimate elements of our food; and accordingly, in reference to such analysis, they arrange vegetable and animal substances under one of the four following heads.

1. Substances composed of carbon, hydrogen, and oxygen, in which the two latter elements are in the same relative proportions to each other, as they exist in water: these, therefore, are sometimes called compounds of carbon and water, or *hydrates of carbon;* this division includes gum, starch, sugar, and woody fibre or lignin.

2. Substances composed of carbon, hydrogen, and oxygen, in which the hydrogen is, as compared with the first class, in excess. These, therefore, are compounds of carbon, water, and hydrogen, or *hydruretted hydrates of carbon;* this division includes fat, oils, resins, and some other highly combustible bodies.

3. Substances in which the oxygen is in excess, and which are compounds of carbon and water, and oxygen, or *oxygenated hydrates of carbon;* such as the greater number of the vegetable acids.

4. Substances composed of carbon, hydrogen, oxygen, and nitrogen; a class which includes albumen, gluten, gelatine, and some other animal and vegetable substances, which, in consequence of containing nitrogen, are characterized by yielding ammonia (a compound of nitrogen and hydrogen), when subjected to destructive distillation.

Having now enumerated the *ultimate elements* of which our food is constituted, and having stated the methods by which their relative proportions are determined, we may proceed to examine its *proximate varieties*, and to show the properties by which they are recognised.

By the term *proximate vegetable principles*, we mean the distinct products resulting from the combination of the ultimate elements; they are distinguished by certain chemical properties which enable us to identify and detect them. To illustrate what may therefore be termed the *proximate analysis* of a vegetable product, we may select that of wheat-flour as an example, and then proceed to detail the characteristic properties of such of the proximate organic principles as are concerned in food and diet.

When wheat-flour is washed under a running stream of water, a tough viscid substance remains on the sieve; and the water which passes through, carries along with it a quantity of fine white powder, which remains for a time suspended, but which it gradually deposits. The former substance is *gluten;* the latter *starch;* and the mixture of these, constitutes *flour;* so that they are called its *proximate principles* or *elements.*

GLUTEN, when moist, is tough, adhesive, and elastic; it is insoluble in cold water, nearly tasteless, and when submitted to destructive distillation, it yields a considerable quantity of ammonia; so that it contains

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A POPULAR COURSE OF GEOLOGY.

V.

NON-METALLIC SOLIDS AND FLUIDS.

SULPHUR.—Sulphur is a brittle solid, of the specific gravity 1·99, or about twice the weight of water. It is of a yellow colour, emitting, when rubbed, a peculiar and well-known odour, but has little taste. It is highly combustible, burning with a blue flame, and suffocating vapour. It is found in many parts of the earth, particularly in the neighbourhood of volcanoes. The largest supplies of it are obtained from the Solfaterra, near Naples, supposed to be the crater of an extinct volcano. It is found, however, much more abundantly in combination with the metals iron, copper, lead, silver, and antimony.

The sulphurets are the commonest ores of most of the metals. Sulphur is procured in large quantities by exposing sulphuret of iron to a red-heat, and collecting the sulphur which sublimes. Combined with oxygen it forms sulphuric acid, which, united with alkalies, earths, and metallic oxides, gives rise to a class of salts called sulphates, which are abundant mineral products. Sulphate of barytes, or heavy spar, and sulphate of lime, or gypsum, are common. Sulphate of strontian is less so. The sulphates of magnesia and soda are frequently contained in mineral springs. Sulphate of iron, or green vitriol, sulphate of copper, or blue vitriol, and sulphate of zinc, or white vitriol of commerce, are occasionally found crystallized accompanying the sulphurets of those metals, but are much more common in the water issuing from mines where the sulphurets are raised in large quantities. A decomposition of the sulphurets takes place, arising from atmospheric agency. Oxygen combines with the metal to form an oxide, and with the sulphur to form sulphuric acid; and the acid and the oxides unite, and produce sulphates.

If a bar of iron be immersed in a solution of sulphate of copper, it will soon be coated with a film of metallic copper. The acid, having a greater affinity for the iron than for the copper, unites with the former, and the latter is precipitated. Much copper is by this process obtained at the Parys mine in Anglesey, and some other mines, where the decomposition of large quantities of ore too poor for smelting, causes the water issuing from the mines to be charged with sulphate of copper, the metal of which is precipitated by the water being made to pass through large tanks filled with old iron. Sulphur is a non-conductor of electricity, and a negative electric. Its combining proportion is 16.

PHOSPHORUS.—Phosphorus is a soft tenacious solid, usually of a flesh-red colour. It is highly inflammable, burning with a pale-yellow flame, and undergoing slow combustion at the ordinary temperature of the atmosphere. The product of its combustion is phosphoric acid. Phosphoric acid enters into the composition of several minerals, namely, the phosphates of lime, lead, copper, manganese, and iron. Phosphorus is a negative electric. Some uncertainty prevails respecting its combining proportion. It is considered to be 18.

Iodine, bromine, and selenium, are of little importance in the mineral kingdom, however interesting on account of their chemical properties. Iodine, which is derived from the ashes of sea-weeds, has never yet been detected in any mineral. At ordinary temperatures it is a soft friable substance, opaque, of a blueish colour, and a metallic lustre. It is a negative electric, and a non-conductor of electricity. It sublimes rapidly at a low heat, with a vapour of a very rich violet-colour, whence its name*. Its combining proportion is 124.

Bromine—This substance was discovered by M. Balard of Montpellier, in 1826. In its chemical relations it bears a close analogy to chlorine and iodine, being always associated with the former, and sometimes with the latter.

It exists in sea-water, and in several mineral springs in Germany, and has been found in the ashes of sea-weeds, and of several plants that grow on the shores of the Mediterranean.

Bromine, at ordinary temperatures, is a liquid of a blackish-red colour when viewed in mass, but of a hyacinth-red by transmitted light. Its action on some of the metals is similar to that of chlorine. It is a non-conductor of electricity, and a negative electric. Its combining proportion has not been ascertained.

Selenium.—Selenium, at common temperatures, is a brittle, solid, opaque body, without taste or odour, of a metallic lustre, having the aspect of lead in mass, of a deep-red colour when reduced to powder. Its specific gravity is between 4·3 and 4·32. The Swedish chemist Berzelius, by whom it was discovered, supposed it to have been a metal; but being a non-conductor of electricity, it is more properly classed with the non-metallic bodies. It may be detected by the peculiar powerful odour, like that of decayed horse-radish, which it emits when heated. It was found mixed with the sulphur sublimed from the iron pyrites of Fahlun in Sweden. It is a negative electric, and a non-conductor of electricity. Its combining proportion is 40.

Boron.—Boron is a dark olive-coloured substance, without either taste or smell, about twice the weight of water. On being heated it suddenly takes fire, oxygen disappears, and boracic acid is formed. It was first obtained by Sir Humphry Davy, by exposing boracic acid to the action of a galvanic battery. Boracic acid, which is therefore a compound of boron and oxygen, occurs as a natural product in the hot springs of Lipari, and of Sasso near Florence. It is also a constituent of two very rare minerals, boracite, or borate of magnesia, and datholite, or borate of lime. Boracic acid is collected in considerable quantities on the shores of some lakes in Thibet and Persia, whence it is imported into Europe, through India, under the name of tincal, which, after being refined, is the borax of commerce. Its chief use is as a flux for some of the metals, for which purpose it is of great importance in the analysis of minerals by the blowpipe.

Boron is a non-conductor of electricity, and a negative electric. Its combining proportion is 8.

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* Ἰώδης,—violet-coloured.

CARBON.—Carbon is the peculiar inflammable principle of charcoal, which is the product of wood heated to redness in close vessels, or under such circumstances as to exclude the air; and the diamond is carbon in absolute purity.

Newton was led to suspect the diamond to be combustible, from its great powers of refraction, and modern chemists have confirmed the conjecture by effecting its combustion. Its product, when burned in oxygen gas, is carbonic acid; and when burned in contact with iron, it converts it into steel, producing the same effects as are produced by charcoal under similar circumstances.

Carbon enters into the composition of coal, bitumen, and amber, and of the inflammable liquids, naphtha and petroleum. Black lead, or plumbago, is a native carburet of iron. But the most important office performed by carbon in the mineral kingdom is as the base of carbonic acid, which is generated when carbon is burned in oxygen gas, and constitutes, as we have before observed, 22 parts out of 50 of that abundant rock, carbonate of lime.

Carbonic acid is absorbed by water, of which it is deprived by being boiled. Hence the agreeable and refreshing flavour of spring-water and the insipidity of water that has undergone the process of boiling. Water saturated with carbonic acid sparkles when poured from one vessel into another, such are the waters of the mineral springs of Tunbridge, Pyrmont, Carlsbad, and many others.

Carbonate of lime is very sparingly soluble in water, but dissolves in an excess of carbonic acid; hence water, charged with that acid, percolating through limestone, dissolves a portion of the rock, which it again deposits by the dissipation of the acid. This is the origin of those pendent masses, like large icicles, called stalactites, which are so abundant in caverns in limestone, and also of calcareous tufa, deposited by springs. Among the most remarkable calcareous springs, are those of San Filippo, in Tuscany, at which a curious manufactory of medallions in basso-relievo is established. The water of these springs is hot, and holds in solution a large quantity of carbonate of lime. After being made to pass through a series of pits, in which its coarser matter is deposited, it is conducted to the summit, a chamber in which numerous cross sticks are placed, to break the fall of the water, and disperse the spray as much as possible. This falls on moulds placed to receive it, and a deposit of solid carbonate of lime is formed upon them, and thus most beautiful casts are obtained. Some idea may be formed of the immense quantity of calcareous matter which these springs contain, from two circumstances mentioned by Mr. Lyell respecting them; namely, that they deposit a stratum of hard stone a foot thick in four months, and that on the side of the hill is a mass of travertin, as this stone is called, a mile and a quarter long, a third of a mile broad, and at least 250 feet thick. The petrifying properties of many springs in England are similar to those of the waters of the baths of San Filippo, though exhibited on a much smaller scale,—the substances said to be petrified by immersion being covered by an incrustation of carbonate of lime. A similar deposit takes place in boilers and tea-kettles, from the dissipation of the carbonic acid by the boiling of the water *causing a precipitate* of calcareous matter. The springs of limestone countries are frequently rendered unwholesome by the quantity of carbonate of lime which their waters hold in solution. But it is not of limestone only that carbonic acid is a solvent. It has a decomposing effect upon many other rocks, particularly of those which contain felspar, and it renders oxide of iron soluble in water. It exudes from the earth, and collects in pits, caverns, and old wells. It is, when respired, destructive to animal life; and, as it is incapable of supporting combustion, its presence, in too great quantities to be breathed without fatal effects, may be detected by the introduction of a lighted candle into the suspected place, which ought not to be entered if that is extinguished. Exhalations of carbonic acid gas are most abundant in volcanic countries. The Grotta del Cane, near Naples, celebrated for the cruel exhibition of its effects on dogs, for the gratification of the curiosity of idle tourists, owes its destructive qualities to the evolution of carbonic acid gas.

Carbon is a negative electric, and a conductor of electricity. Its combining proportion is 6.

We come now to the alkaline metals, potassium, sodium, and lithium. Until the year 1807 the two fixed alkalies, potash and soda, (for lithia had not then been discovered), had baffled all attempts of chemists to decompose them. The discovery of their elements was a triumph reserved for Davy, who, wielding the potent energies of the voltaic pile, evoked, as with the wand of an enchanter, a new class of metals out of substances the metallic nature of which had not till then been suspected; and the same potent spell in the hands of Crosse is now working wonders nearly as great, effecting the composition of simple minerals, to produce which out of their elements had hitherto been attempted in vain, and is thus revealing to us one of the most curious and secret processes of nature, the formation of metallic veins. But if we liken the galvanic battery to the wand of a magician, the metals evoked by it may be compared to spirits obeying with reluctance the summons that called them to the light of day, and vanishing almost as soon as they appear; or, to drop the language of metaphor, their affinity for oxygen is so great that they return to the state of an oxide in a few minutes, and they are with difficulty preserved in the metallic state long enough for the investigation of their characteristic properties.

Potassium.—When hydrate of potassa, slightly moistened, is connected with the opposite poles of a galvanic battery, the oxygen, both of the water and the potassa, passes over to the positive pole, while hydrogen and potassium appear at the negative. Potassium is solid at the ordinary temperature of the atmosphere, becomes partially fluid at 50°, and completely so at 150°. In colour and lustre it resembles mercury, but differs from other metals in being lighter than water. Its powerful affinity for oxygen has already been spoken of. It oxidizes rapidly in the air, or in fluids containing oxygen. It decomposes water the instant it touches it, the potassium being inflamed and burning with a vivid light while swimming on the surface. It can only be kept from oxidation by being kept in glass tubes hermetically sealed, or by being immersed in liquids which do not contain oxygen as one of their elements. Its combining proportion is 40. Potash, or potassa, the oxide of potassium, is found combined, in small quantities, with nearly twenty earthy minerals, among others, with mica and felspar. It also combines with some acids. Nitrate of potash, or saltpetre, has already been spoken of as a mineral product under the head of Nitrogen.

Sodium.—Sodium was discovered by Sir Humphry Davy shortly after the discovery of potassium. It has a strong metallic lustre, like that of silver, and is so soft at the common temperature, that it may be pressed into leaves with the fingers. It soon oxidizes when exposed to the air, though less rapidly than potassium. When thrown into water it swims on the surface with a violent hissing noise, and is rapidly oxidized, hydrogen being evolved, but no light is visible. It possesses the characteristic properties of other metals, in being a positive electric. Its combining proportion is 24. Soda, the oxide of sodium, is found in variable proportions in about twelve earthy minerals, but in none that are metallic. It combines abundantly with several acids. The borates, carbonates, sulphates, and muriates of soda, have been already noticed, under the heads of boron, carbon, sulphur, and chlorine.

Lithium.—A new alkali, to which the name of lithia was given, was discovered in the year 1818, by a Swedish chemist, in analyzing a mineral called petalite; it has also been found in spodumene, and in several varieties of mica. Lithia was decomposed by Sir H. Davy, by the same means which had effected the decomposition of potash and soda, and a white metal resembling sodium was obtained, which was re-oxidized, and thus converted into lithia, so rapidly that it could not be collected. Its combining proportion is 10.

Metallic Bases of the Alkaline Earths.

The four earths, barytes, strontia, lime, and magnesia, possess alkaline properties, but they are less caustic than the alkalies, are not fusible, except by voltaic electricity, or the hydro-oxygen blowpipe, are less soluble in water than the alkalies, and form, with carbonic acid, insoluble compounds. These earths are reduced to the metallic state with much greater difficulty than the alkalies, and the properties of their bases have not been so well ascertained.

Barium.—Sir H. Davy obtained this metal by mixing carbonate of barytes in a paste with water, and placing a globule of mercury in a hollow on its surface, and then bringing it in contact with the opposite poles of a galvanic battery. The barium of the barytes formed an amalgam with the mercury, and on submitting it to heat in close vessels, from which the air was excluded, the mercury sublimed. Barium has only been obtained in small quantities, and its properties have therefore scarcely been determined with precision. It is of a dark-gray colour, with less lustre than cast-iron, and its specific gravity is greater than that of water. It attracts oxygen rapidly from the atmosphere, and yields a white powder, which is baryta. Barium effervesces strongly in water, with disengagement of hydrogen. Its combining proportion is 70.

Baryta or barytes*, so called from the weight of its compounds, is a gray powder about four times heavier than water. It has a sharp alkaline taste, converts vegetable blues to green, and neutralizes acids. All the soluble salts of baryta are poisonous. Sulphate of baryta is one of the most insoluble substances with which the chemist is acquainted. Baryta is by no means a very abundant earth. Its salts communicate a yellow-colour to flame.

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* *βαρυς*,—heavy.

STRONTIUM.—This metal was discovered by Sir H. Davy, by a process similar to that employed to obtain barium. Little more is known respecting it than that it is a heavy metal resembling barium, and that it is converted into strontia by contact with water or air. Its combining proportion is 44. Strontia, like the other earths, is white, when dry and pure, and resembles baryta in its infusibility and alkaline properties. The salts of strontia, when dissolved in alcohol and inflamed, tinge the flame of a blood-red. Strontia occurs native combined with carbonic and sulphuric acids. Its name is derived from Strontian, in Scotland, where the carbonate was first discovered.

CALCIUM.—The existence of the metallic base of lime was proved by Sir H. Davy, by the means before described as employed for the decomposition of baryta and strontia. It is whiter than barium, and is converted into lime by oxidation. Its other properties are unknown. Its combining proportion is estimated at 20.

Lime, or oxide of calcium, has never been found pure; when prepared by the chemist it is white and caustic, and changes vegetable blues to green. It is infusible, except by the hydro-oxygen blowpipe, or by voltaic electricity. When a stream of oxygen gas is directed upon red-hot lime, it produces a light too intense to look upon.

Lime is a most important and abundant earth. It is prepared in large quantities by exposing the carbonates of lime, such as limestone, marble, chalk, calcareous spar, or sea-shells, to a red heat, when the carbonic acid is driven off and lime remains. In this state it is caustic, and is called quick-lime. When moistened with water it falls to powder and combines with the water. It then becomes a hydrate of lime, commonly called slaked lime, in which state it is used for mortar.

The importance and utility of lime and its compounds, is as great as its distribution is extensive. Its use as a manure, a cement, and for many other purposes in the arts, is too well known to require enumeration. Besides the carbonate, lime occurs as a fluate, borate, and arseniate.

MAGNESIUM.—The existence of this metal was likewise proved by Sir H. Davy, though he obtained it in quantities too minute for ascertaining its properties, any further than that it decomposes water, and that its oxide is magnesia. Its combining proportion is 12.

Magnesia is a white powder, and, when pure, has neither taste nor odour. It is of the specific gravity of 2·3, and very infusible. It forms a hydrate with water, for which, however, it has a less affinity than lime. It is sparingly soluble in water. The solution of magnesia is inert with respect to vegetable blues, but when pure magnesia is laid on turmeric-paper, it causes a brown stain. This is a decided proof of its alkalinity.

and it possesses another essential alkaline property, that of forming neutral salts with acids.

Magnesia is found in about thirty minerals, but they are not so abundant as in the compounds of lime or silica, nor does it form the prevailing ingredient in any of them. Minerals containing magnesia have frequently a soapy feel. It is found in combination with the carbonic, sulphuric, and boracic acids. The mineral in which it exists in the greatest purity is the hydrate, called also native magnesia.

THE NON-ALKALINE EARTHS.

ALUMINUM.—From some experiments of Sir H. Davy, it was inferred that the earth alumina is a metallic oxide; and aluminum, the metal, has lately been procured in a pure state by Dr. Wöhler. It is a gray powder, very similar to that of platinum, occurring, generally, in small scales of a metallic lustre. It requires for its fusion, a higher temperature than that at which cast-iron melts. When heated to redness in the open air, it takes fire and burns with a vivid light; aluminous earth, of a white colour and considerable hardness, being the result. When heated to redness in a vessel of pure oxygen, it burns with intense light and heat, the resulting alumina is partially vitrified, and equal in hardness to corundum, an aluminous crystallized mineral.

Water, at common temperatures, does not oxidize aluminum, but oxidation commences when the water is heated to near the boiling-point; and a feeble disengagement of hydrogen takes place. Aluminum is not a conductor of electricity in the state of powder, but becomes so when fused. The earth alumina, in its pure state, has neither taste nor smell, is quite infusible, and insoluble in water, for which, however, it has a powerful affinity, forming with it a hydrate. It attracts moisture from the atmosphere, and adheres strongly to the tongue. When once combined with water, it cannot be rendered anhydrous, except by a white-heat.

Alumina is a very common earth; it occurs in every country, and in rocks of all ages, and the aluminous rocks are among the most abundant in every formation. It is an important ingredient in soils, imparting to them tenacity and the power of retaining moisture. Those soils in which it forms the chief ingredient, are termed stiff, or clayey, and are best adapted to the growth of wheat.

When mixed with water, hydrate of alumina forms a soft plastic mass, easily moulded into various forms, and capable of being hardened in the fire, and hence its utility in the manufacture of pottery. Although hydrate of alumina, or clay, is a very abundant mineral product, yet in a state of purity it is extremely rare, being generally contaminated with silica, lime, or oxide of iron, the presence of which latter ingredient causes the red colour of most kinds of clay when burned. Marl is a mixture of clay with calcareous matter. Rocks in which alumina prevails may be recognised by the earthy smell which they give out when breathed upon. This is called the argillaceous odour, though it does not belong to alumina in a state of purity. Alumina occurs combined with the hydro-fluoric and sulphuric acids, and with potash and soda. Though the most common form of alumina is a rude, amorphous, soft clay, it likewise occurs crystallized. Those beautiful gems, the ruby and the sapphire, consist almost wholly of alumina, and rival the diamond in hardness and in value. This earth likewise amounts to 86 per cent in corundum or emery, which is so hard as to be employed in polishing steel, and in cutting and polishing gems. The name of alumina is derived from alumn, of which it is the base.

SILICUM.—The metallic base of the earth silica was obtained by Sir H. Davy, by bringing the vapour of potassium in contact with pure silica heated to whiteness, when a compound of silica and potassa was formed, through which the base, silicum, was diffused in black particles like plumbago. Nevertheless, much doubt existed whether it was a metal, and some chemists classed it among the non-metallic bodies, under the name of sili*con*. The recent researches of Berzelius appear nearly decisive of this point. He first obtained the base, silica, in a state of purity, and found it a solid, of a dark nut-brown colour, without the least metallic lustre; and it wanted another characteristic of a metal, that of being a conductor of electricity. Its combining proportion is 8.

Silica is the most abundant of the earths. In its pure state silica is a light white powder, without taste or odour, and feels rough and dry when rubbed between the fingers. It is infusible, except by the oxy-hydrogen blowpipe, to which, however, it yields more readily than either lime or magnesia. Silica is quite insoluble in water, which, however, dissolves it in large quantities in its nascent state. In order that silica should be held in solution in springs, it appears necessary that the water should be at a high temperature. The most celebrated hot siliceous springs are those of St. Michael, one of the Azores, and of the Geysers in Iceland, from each of which is deposited a siliceous incrustation called sinter, analogous to the calcareous tufa which we have before described as resulting from waters containing carbonate of lime.

Silica appears to possess the properties of an acid rather than an alkali. No acid acts upon it except the hydro-fluoric, but it is dissolved by solutions of the fixed alkalies, and combines with many of the metallic oxides. Some chemists, therefore, among whom is Berzelius, denominate silica *silicic acid*, and its compounds with alkaline and earthy bases and oxides, *silicates*. The compound earthy minerals containing silica, are in this view native silicates.

When silex, or pure sand, is fused with carbonate of soda or potassa, carbonic acid is expelled, and a silicate of the acid produced; the nature of which varies according to the proportions of alkali employed. If the compound consist of one part of silica and three parts of alkali, a vitreous mass will be formed, soluble in water; but on reversing the proportions, a brittle transparent compound is formed, not soluble in water, nor in any acid except the hydro-fluoric. This is glass. Every kind of glass is composed of silica and an alkali, and the quality of it depends on the purity or impurity of the materials used. Silica is a component of two-thirds of the earthy minerals, whose composition is known. As quartz-rock, it forms whole mountain-masses. It is the principal ingredient of sandstones. Rock-crystal, calcedony, flint, and jasper, consist almost wholly of it, and silica of sufficient purity for most purposes may be procured by heating rock-crystal, throwing it when red-hot into water, and then reducing it to powder. The name of silica is derived from *silex*, the Latin for flint.

The remaining earths, yttria, glucina, and zirconia, occur too rarely, and are consequently too little known, to require or admit of much description. They are supposed from analogy to have metallic bases like the other earths, but they have never been decomposed.

Yttrium is the supposed base of the earth yttria, which was discovered as a component of a mineral called gadinolite, found near Ytterby, in Sweden, whence the name of yttria. Sir H. Davy demonstrated that oxygen entered into the composition of yttria, by bringing it when ignited into contact with potassium, when the oxygen of the yttrium united with the potassium and formed potassa, and dark metallic particles were diffused through the alkali. It is found only in one or two rare minerals besides the gadinolite. Its combining proportion is estimated at 36.

Glucina derives its name from the Greek word γλυκυς, *sweet*, from the sweet taste of its salts. When pure, it is a white powder without taste or odour, soft, and quite insoluble in water. The metallic nature of its base has been inferred on the same grounds as that of yttria, and its combining proportion has been estimated at 36. Glucina occurs only in small quantities, combined with other elements in the minerals euclase, beryl, emerald, gadinolite, and topaz.

Zirconia is an earth discovered in the zircon of Ceylon, and the hyacinth of Expailly, in France, as a white powder (when pure) resembling alumina in its appearance, and without either taste or odour. Its specific gravity is 4·3. Like the other earths, it is infusible by ordinary means. The experiments of Sir H. Davy led to a presumption that its base was of a metallic nature, and Berzelius has since procured it in an insulated state, in the form of black powder. Its metallic nature is very doubtful; it may be pressed out into shining scales, but has not been procured in a state capable of conducting electricity. Its combining proportion appears to be 40.

The substance called *thorina*, which Berzelius supposed to be a distinct earth, he has since found to be a phosphate of yttria.

The twenty-nine metals which close the list of simple bodies, are to be considered rather as accidents of rocks than as their essential ingredients. For this reason, although they are a most useful, important, and interesting class of substances, we shall not enter into a description of them now, reserving what we have to say on that subject till we treat of metallic veins. When it is said that they are not essential ingredients of rocks, an exception must be made with respect to iron, the oxide of which is of almost universal distribution, there being scarcely a mineral in which it is not found in small quantities. With respect to the relative abundance of the earths, silica is the most common, alumina the next so. Lime occurs among the earlier rocks, but is most abundant in the stratified secondary series, and with the increase of lime a corresponding increase appears to have taken place in the number of testaceous molluscs.

A POPULAR COURSE OF CHEMISTRY.

No. VIII.

Gases.—Hydrogen.

We paid no particular attention to the quantity of our materials when decomposing *water* by *zinc* or *potassium ;* and although I am aware that no great accuracy of result can be obtained by the juvenile student, even if he works by weight and measure, yet I think it may be as well, at the outset of this essay, to show how the theory of definite proportions is illustrated by the decomposition of water, in the two instances alluded to, at page 369 of this volume.

In order to render the matter as simple as possible, I will first of all suppose that *single proportionals*, or *single equivalents* of the acting bodies are concerned, and these will be, water = 9, zinc = 32, sulphuric acid = 40. Remember, that I am now exclusively speaking of *weights.* Now *water* consists of 1 *hydrogen* + 8 *oxygen* = 9; *zinc* is an elementary body, a metal. It has been already shown that metals are capable of combining with oxygen, or of becoming *oxidized*, and supposing that I wish to convert 32 of zinc into an *oxide*, I find that 8 of oxygen are requisite to produce such result, and to furnish 40 of *oxide of zinc*, which of course is = 32 + 8.

If I put 32 of zinc to 9 of water, no action or oxidizement ensues; although the water contains 8 of oxygen, which is the exact quantity that the zinc has an affinity for under other circumstances, but here it cannot expel the 1 of hydrogen, from its combination with the 8 of oxygen; but if I now add 40 of sulphuric acid, a singular and most remarkable play of affinities is instantly induced; the 1 of hydrogen is expelled from combination with the 8 of oxygen, which unites with the 32 of zinc, forming 40 of oxide of zinc, and this is attracted by the 40 of sulphuric acid, producing 80 of sulphate of zinc.

The sulphuric acid undergoes *no decomposition*, and its use in the experiment is to combine with the oxide of zinc, as fast as it is formed at the expence of the oxygen of the water. Now then, with regard to the decomposition of water by potassium, in this instance the acting bodies are, water = 9, potassium = 40. In order to oxidize 40 of potassium, 8 of oxygen are required to furnish 48 of oxide of potassium or potassa, which of course is = 40 + 8; here, as in the former instance, the water contains the necessary 8 of oxygen, but no acid is required to promote the action of decomposition, for potassium has a most intense affinity for oxygen, so that the instant the globule of metal touches water, the 1 of hydrogen is expelled, and the 8 of oxygen combine with the 40 of potassium, producing 48 of *oxide of potassium or potassa.*

Such, then, is the theory of these two processes for evolving hydrogen by the decomposition of water, and such are the proportions of the acting bodies capable of entering into combination without any excess or deficiency. But in the ordinary process of evolving hydrogen for the purpose of experiment, there is always a great excess of water present, which is requisite for two reasons, namely to dilute the sulphuric acid, and thus modify its intense affinity, and also to dissolve the sulphate of zinc. Sulphuric acid also, in its most concentrated state, always contains water, but this is thrown off when it unites to the oxide of zinc. The term, "throwing-off water," is a laboratory term, and in common use amongst chemists. You must not, however, imagine that you can actually see this taking place,—far from it; for the fact of the acid throwing-off its water, can only be ascertained by very delicate experiments.

In decomposing water by potassium, there is always great excess of water present, and in this excess the oxide of potassium dissolves, furnishing you with an alkaline solution of potassa.

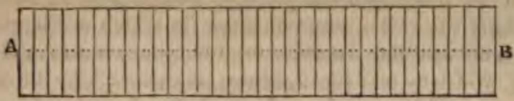
The important part then to be remembered, is briefly this:—whenever you take 32 parts of zinc, and 40 parts of sulphuric acid, (and it matters not, whether grains, ounces, pounds, hundreds, or tons,) and cause these to act on water, in any quantity above a single proportional, say three proportionals = 27, you may be sure that one proportional = 9 is decomposed, its hydrogen expelled, in the free gaseous form, and its oxygen united to the zinc, forming 40 oxide of zinc, and this to the 40 of sulphuric acid, forming 80 of sulphate of zinc, which dissolves in the remainder of the undecomposed water, and may be obtained in the crystalline state, after the manner already directed.

It is in consequence of oxygen thus combining with the metals employed for decomposing water, that you never, by any chance, can collect it in its gaseous state; hydrogen is the only gaseous result; but by the agency of voltaic electricity, we can effect the decomposition of water in a most beautiful and satisfactory manner, and exhibit the evolution of both of its elementary gases. The manipulations requisite to ensure the success of this experiment are now to be described.

Very little progress can be made in practical chemistry unless you possess a considerable share of mechanical skill; I do not mean the refined mechanical skill of a mathematical instrument maker, an optician, or a watchmaker; but a good sound knowledge of the first principles of mechanical science, and the dexterous use of a small set of tools, comprising a saw or two, a plane, chisels, hammer, &c. Thus furnished, you can immediately make your apparatus, and submit it to the test of experiment, without being at the trouble of drawing out a plan, then explaining it all to a workman, and perhaps waiting day after day for his tardy execution of your orders. Much valuable time and much expense are saved, by the chemist becoming, as far as possible, his own mechanic.

The voltaic battery is required for the decomposition of water; you can purchase it ready-made if you please; but you can very easily make one, which, although perhaps not quite so neat and showy, will answer all your purposes. I will therefore instruct you how to set about the matter, for the apparatus will be found very useful in a variety of experiments which I shall have to bring before you at a future period of our "Course of Chemistry."

Take a piece of half-inch mahogany board, about 2 feet long and *5 inches wide, plane* it up smooth on both sides, and then at intervals of three quarters of an inch apart make grooves right across it; these grooves may be an eighth of an inch wide and deep.



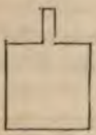
This being done, cut the board in half, lengthwise, in the direction of the dotted line from A to B, and plane up both edges; then take another piece of mahogany of the same thickness and length, but only three inches wide; smooth this up; it is to form the bottom of the voltaic trough, and the other pieces are to form the sides; these are now to be firmly nailed or screwed on to the bottom, so that the grooves face each other, and the space "in the clear" inside must be two inches. Thin pieces of mahogany must now be prepared, so as to slide into the grooves, and form a number of distinct cells; these partitions must reach from the top to the bottom of the trough, and when you have fitted them all, number each, in its place, with a pencil, and then remove them all, excepting one at each end.

The trough has now to be cemented, and the cement required is made of three parts of rosin, and one part of bees' wax, and sufficient Venetian red to give it a good colour and consistence.

The rosin must be melted in an iron ladle, over a slow fire; the wax is then to be added, and lastly the Venetian red, which you should previously dry, in a shovel over the fire, just as I formerly directed for the oxide of manganese, at p. 184, vol. ii. This precaution of drying is necessary, because it is an earthy colour, containing a great deal of water; melted rosin has a very high temperature, and if the damp colour were thrown into it, the water would be converted into steam, and the contents of the ladle would boil over, and very likely produce a dangerous accident. With dry Venetian red no accident can happen; it must not all be thrown in at once, but a little at a time, and keep stirring the while with a flat piece of wood. The use of adding this or any other earthy colour to the rosin and wax, is to give a firm consistence to the cement when cold. Just the same sort of thing, only much coarser, is done to fix the shanks of knives and forks into their handles; the cement commonly employed is melted rosin and brick-dust, the latter giving strength to the former.

The colour being properly stirred in, and the cement very hot, by means of a stick dipped into it, apply some to the two end partitions of the trough, so as to make them fit their grooves perfectly water-tight. And now, having done this, pour some very hot cement into the trough, to the depth of about half an inch; then quickly slide all the partitions into their respective grooves, and before the cement has time to chill, hold the trough at each end, and incline it from you so that the cement may flow up to the top of the side furthest from you, and then quickly incline it in the opposite direction, so that the side nearest to you may also become cemented. If the cement should happen to chill before you can effect this, you must take a hot, thick, iron wire, and applying it to the cell, cause it to run against the partitions and their grooves. Each cell must be treated so; for the object is to make them all water-tight. It is as well also to cover the partitions themselves with a thin layer of cement, and this can be done by a flat piece of heated iron, to melt cement on them, something after the same manner in which a druggist spreads a plaister with a hot spatula. In order to prove your success in this operation of cementing, you may test the tightness of each cell, by filling each separately with water; if it oozes through, mark the place, empty out the water, wipe it dry with a cloth, and melt some cement over the leak; and all leaks being stopped, the trough of the battery is completed. Now then for the battery itself; and the materials for forming it are *zinc* and *copper*. You require these metals in sheets, and there is no difficulty in procuring them in this state. It is as well to get a tolerably stout sheet of zinc, about the sixteenth of an inch thick, but very thin sheet copper will suffice. In the first place, ascertain what space you have "in the clear" in the cells, it is probably not two inches, but somewhat less, on account of the cement taking up some room; cut a bit of stiff card so that it will slip easily into each cell in the direction of the partitions, and cut it off, at the top of the partitions; this will give you a pattern by which you can cut your zinc and copper plates. You will require thirty zinc plates; therefore mark out the proper number on the sheet of zinc exactly the size of the card pattern and with a large pair of strong scissors, or shears, cut them out; they will curl a good deal in doing this, and therefore, after they are cut, you must flatten them with a hammer, on a smooth stone or anvil. Mark out a similar set, on the copper plate, but with the addition of a small tongue to each, about half an inch wide, and an inch long, like the annexed figure (1), and flatten these out if necessary. Now bend the tongue of each, as in fig. 2, and get a tinman to solder the end of each to a zinc plate, thus, (fig. 3), so that the compound plates will exactly drop over the partitions of the cells of the trough, after the manner represented in this section, which is intended to represent a few cells of the trough; the broadest lines representing the wooden partition, the next broadest, the zinc, and the narrowest, the copper plates; they are to hang over the partitions without touching each other. You must get this soldering done by a tinman, for it is an art not very easily acquired, especially where zinc is concerned, for it spoils the proper flowing of the solder, and even the tinman will tell you it is a very "difficult job:" but it *can* be done, and be sure that the plates are really soldered, and not merely stuck together with the melted rosin used in the operation, which is sometimes the case. *Perfect metallic contact* is essential to the success of your apparatus.

1



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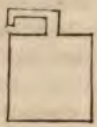

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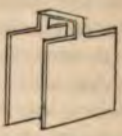
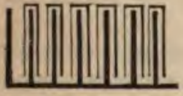
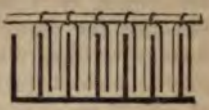
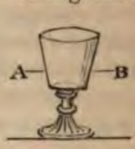




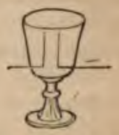
Well then, all this being attended to, and each compound plate fitted to a cell, lay a slip of dry mahogany, about half an inch square, and two feet long, over the plates, so as to rest on the tongue of each, and then with some fine packthread, well waxed, bind each compound plate by its tongue, to the slip of wood, thus;—observing that all the zinc plates follow the same order, either to the right or to the left hand. Now the object in thus fastening the plates to the slip of wood is, that they may be all lifted out of the cells, or dropped into them, simultaneously; and having effected this arrangement, the voltaic battery is complete; and by its agency, you will be able to decompose water, in the following simple apparatus:—



Take a very small wine-glass, in fact a toy wine-glass, that will hold about a table-spoonful of water, and drill two holes in its sides, exactly opposite each other. This may be very easily done, by means of a four-sided, sharp-pointed, steel tool, called a "rimer," something like a brad-awl, but of harder steel; you can obtain it at any tool-maker's. Suppose this figure to represent the miniature wine-glass:, you have to drill holes through it at A and B. Wet the glass at these spots with turpentine, common spirits of turpentine, and then press the sharp point of the "rimer" strongly against it, and work the "rimer" round and round, just as if you were boring a hole through a piece of wood, and you will find that you thus bore a hole through glass with nearly equal facility. A large hole is not required, but only one of about the 30th of an inch.



Having perforated the glass, it must be washed with soap and water, or a little solution of potassa, to remove the turpentine, and afterwards wiped dry. Into each of these holes a piece of thick platinum wire must be inserted, and firmly cemented outside, with the same cement already spoken of, so as to appear like the annexed figure; and then the glass is to be filled with water, containing ten or twelve drops of strong sulphuric acid.



This water you have to decompose by electricity, and the only reason why the small portion of sulphuric acid is added is, to render the water a better conductor of electricity, it has no other influence; and therefore you may regard the result of the experiment exactly as if it was performed with pure water,—but of this presently.

The source of electricity in the voltaic battery is chemical action, and to bring this into play, the battery has to be charged. Ascertain how much water the trough of the battery will hold, empty this into a basin, and add to it some sulphuric and nitric acids, so that it produces a slight effervescence on a bit of zinc; then pour this weak acid into the cells of the trough, until they are nearly filled, say within a quarter of an inch—but, at all events, not so full as to cause the liquid to overflow from one to the other when the plates are immersed; remember this,—and you can easily ascertain it beforehand, by putting in the plates first, and then pouring in water until it rises within a quarter of an inch of the top of each cell.

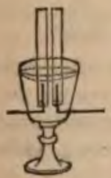
The trough being charged with the weak acid, take two pieces of thin copper bell-wire, each about 18 inches long, and perfectly brightened with emery or glass-paper; and put the end of one into the last cell of the trough on your right, and that of the other into the left, and now place in the plates of zinc and copper. You find chemical action instantly taking place between the acid and the zinc, and this chemical action you have to avail yourself of for decomposing the water, by the electricity which is at the same time developed. Connect the other ends of the copper wires with the platinum wires of the decomposing apparatus, and you will instantly find myriads of small bubbles of gas issuing from each. These are respectively *hydrogen* and *oxygen*, the two constituent gaseous elements of water.

Electricity, then, has the singular power of overcoming the mutual affinity of bodies; for here, when it passes through water, the elements become repulsive of each other, and are each liberated in the gaseous form.

Oxygen here makes its escape at the end of the platinum wire, connected with the *zinc* end of the battery, and it is evolved because platinum has no affinity for it; but substitute for platinum any other metal save gold, and you will find no evolution of gaseous oxygen; but as fast as it is liberated, it will combine, and produce an oxide of the metal.

The end of the battery at which the evolution of the *oxygen* takes place is called the *anode*, and that at which the evolution of *hydrogen* takes place, the *cathode*. In electro-chemical language, therefore, oxygen is called an *anion*, hydrogen a *cathion*.

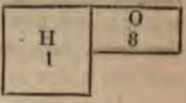
You may, perhaps, say, it is all very true that gaseous matter is escaping from these two points, which you are pleased to call *anode* and *cathode;* but what evidence is there to prove that such matter is oxygen and hydrogen? There is abundance. Take two small glass tubes, about a quarter of an inch in the bore, six inches long, and closed at one end, which may be very easily done by holding the ends in the flame of a spirit-lamp until the glass softens, and then twisting or sealing it together; and when these are cool fill them with water, and invert them into the wine-glass over the platinum wires, just as you would invert a larger tube, or jar, in the pneumatic trough, so as to make an arrangement like this; and now upon making the connexion with the battery, the gases will be collected in the tubes.



Try the gas at the *anode* with the red-hot cinder of a bit of wood or paper, it will be rekindled, proving the presence of *oxygen;* try the gas at the *cathode* with a lighted slip of wood or paper, and it will inflame, proving the presence of *hydrogen.*

Arrange the decomposing apparatus as before, and again make the connexion with the battery. Watch the evolution of the gases from the *anode* and *cathode*, and you will find that *one* volume of *oxygen gas*, and *two* volumes of *hydrogen gas*, are respectively collected in the tubes inverted over the *anode* and *cathode*. Or if you please to call the volume of hydrogen = 1, that of the oxygen will be = ·5, or $\frac{1}{2}$.

The relative specific gravities, or weights, of equal volumes of *hydrogen* and *oxygen*, are as 1 to 16, they do not combine volume for volume, *but in the proportion* of a volume to half a volume; or, if we reduce this to weights, we find that the combining weights are 1 hydrogen + 8 oxygen = 9 water.



The annexed diagram may, perhaps, still further illustrate this important matter. H, representing *one volume* of *hydrogen*, equal in weight to 1, and O a *half volume* of *oxygen*, equal in weight to 8; and these are the proportions in which the two gases invariably combine to produce 9 of *water*, or, strictly chemically speaking, *protoxide of hydrogen*.

These, and the experiments already detailed, will be sufficient for the present to illustrate the important fact of the composition of water. I should, however, state, that if the action of the battery flags during your operations, you must take out the plates, and empty out the charge of acid, which is exhausted and useless; replace it by a fresh charge, wipe all the plates clean, brighten up the connecting wires, and then immerse the plates, which you will find to regain their former activity in effecting the decomposition, or, as it is now very generally called, the *electrolysis* of water. Thus much for the combination of *hydrogen* with *oxygen*. I have now to submit to your attention the combination of *hydrogen* with *chlorine*.

Take a large bottle-full of chlorine, and fill a bladder full of hydrogen, attach to this the blow-pipe tube mentioned at p. 190; set the bottle on the table, with its mouth upwards, and hold the blow-pipe tube pointing downwards; open the cork, compress the bladder gently, and let an assistant kindle the gas at the orifice of the tube: the flame is to play downwards,—not a very large flame; let him then take out the stopper of the bottle of chlorine, and then you must immediately insert the hydrogen flame; its combustion is now supported by chlorine, with great energy; you find the yellow colour of the chlorine quickly disappearing, and a quantity of white fumes making their appearance, which are those of the compound of hydrogen and chlorine, namely, *muriatic acid gas;* now remove the jet, and put into the bottle a bit of wet litmus paper, its blue colour is instantly and powerfully reddened; thus proving the acid nature of the result of the experiment.

You will remember that *chlorine* alone would entirely *bleach* the test-paper, and that *hydrogen* alone would have *no action* upon it. Here, then, is an instance of an *acid*, produced by the union of these two gases.

You now begin to see yet more fully than heretofore the absurdity of calling *oxygen* the *universal acidifying principle;* it produced no acid with burning hydrogen, but only water; here, however, is *chlorine*, an element in which *no oxygen* can be proved to exist, not only supporting combustion, but also producing an *acid*.

The production of *muriatic acid* can be shown in another way, and a sunshiny day is best for this experiment:—Take a stout glass tube, about twelve inches long and an inch in diameter, closed at one end; fill it with clean water, just as you would do a bottle or air-jar in the pneumatic trough; close the shutters of the laboratory, and proceed, by candle-light, as follows:—Transfer into the tube some *hydrogen*, until it is half full, and then transfer into it some chlorine, so as to fill the remaining half; this of course will be *equal volumes* of the respective gases; and they exert no action upon each other, but remain as a simple *mixture;* hold the tube firmly, with its aperture well under the water; and now have the shutters gradually opened; the moment that day-light falls on the tube, you will find a cloudiness appear within it, and the water at the same time will ascend.

Here, then, is an example of the agency of solar light in promoting chemical action; the cloudiness in the tube is owing to the production of *muriatic acid gas*, by the union of *hydrogen* and *chlorine;* the ascent of the water within the tube shows you that the volume of the gaseous mixture is now diminishing in consequence of such *chemical* combination. The light of the candle (or, indeed, of any other common means of illumination,) is unable to excite chemical action between the gases*.

If the sun is shining brightly, and you unclose the shutters, so as to let a solar beam fall suddenly on the gaseous mixture, chemical action is instantly excited; the white fumes appear in abundance; the water rapidly rises to the top of the tube; and very frequently so sudden and vehement is the chemical action thus excited, that the gases combine with explosion; hence the precaution of using a stout glass tube, of holding it firmly, and its upper end inclined away from the face; never make the experiment on large quantities of the mixture, for very often mere day-light will cause explosion. Supposing, however, that the combination has taken place quietly and successfully, remove the tube full of water, and holding its open end upwards, put in a bit of litmus paper, it is instantly reddened, and reddened by *muriatic acid*, because those white fumes were *muriatic acid gas*, which is soluble in water, as indicated by the rise of that fluid in the tube; you have thus obtained an *aqueous solution of muriatic acid;* and that which you purchase at the shops, under the name of *muriatic acid*, is water, holding a more considerable quantity of *muriatic acid gas* in solution.

This commercial acid however is not prepared by the direct union of the gases, but by the decomposition of compounds containing the gaseous elements, as will presently appear.

As *muriatic acid gas* is so readily soluble in water, it is evident to you that it cannot be collected in the *hydro-pneumatic apparatus*, or water-trough; chemists accordingly resort to a pneumatic trough filled with *mercury*, and by means of this contrivance, which is called the *mercurio-pneumatic apparatus*, and used exactly as the other, the gas (and indeed many other soluble gases) may be collected, and its properties examined. But chemists gladly dispense with the mercurio-pneumatic apparatus whenever there is an opportunity of so doing, because a very large quantity of mercury is required, which is very expensive, so that a large trough properly filled, costs between twenty and thirty pounds, and, independent of this great outlay of money, there is much risk and inconvenience in experimenting with jars and bottles of any size when filled and inverted in mercury, on account of its enormous weight, which is more than thirteen times heavier than an equal bulk of water.

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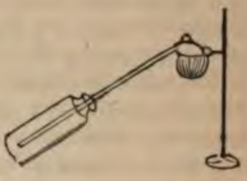
* The brilliant light of the voltaic discharge between charcoal points will effect it, as *Mr. Brande has shown.—Phil. Trans.* 1820.

The following method is therefore had resort to, and we will make the experiment with *muriatic acid gas.*

Take a small tubulated retort with a long neck, and put into it about a table-spoonful of dry common salt, which is a *chloride of sodium*; fold up a piece of white blotting-paper the size of this page, into a long slip, which, thrust up into the neck of the retort; prepare some glass-stoppered bottles, perfectly dry inside, and grease the stoppers. Now place the retort on a retort stand, and with its beak inserted into one of the bottles as here shown; then put a long funnel into the tubulure, and pour in *sulphuric acid* sufficient to moisten the common salt; quickly remove the funnel, and insert the greased stopper of the tubulure.



An effervescence takes place in the body of the retort, which is due to the evolution of *muriatic acid gas*, and as it is much heavier than air, it will pass over and displace all the common air in the bottle; put a bit of litmus paper near its mouth, and, by the intensity of its reddening, you can pretty well judge when all the common air is expelled, and the bottle full of *muriatic acid gas.* Then slide away this bottle quickly, stopper it, and place another to be filled, and so on, until no further action takes place in the retort.

The heat of a spirit-lamp is sometimes necessary to help the evolution of the gas, but it must be applied cautiously, or perchance the materials will effervesce over, and spoil the operation.

The use of the blotting paper is to absorb any water that may chance to distil over.

Well, then, thus you collect *muriatic acid gas* without the aid of a mercurio-pneumatic apparatus; let us now proceed to examine its properties a little more in detail, but chiefly as regards its composition.

Take a piece of potassium, place it in a deflagrating spoon, heat it over a spirit-lamp until it burns, and then quickly plunge it into a bottle of muriatic acid gas, the metal goes on burning vividly, and at the same time you find a lambent flame playing out of the mouth of the bottle; when the combustion ceases, there remains in the spoon a portion of a white saline matter, and not a trace of *muriatic acid gas* is discoverable, provided enough potassium has been employed.

In this beautiful experiment, the decomposition of *muriatic acid gas* is effected by the strong affinity of the hot *potassium* for the *chlorine; chloride of potassium* is produced, and left in the spoon, whilst the *hydrogen* is liberated in the free gaseous state, and inflames by the intensity of the action, and burns at the mouth of the bottle.

From this and other analogous experiments, conducted with all due attention to accuracy, it is found that *equal volumes* of *chlorine* and *hydrogen*, combine to produce *muriatic acid gas;* the weights of these volumes are as 36 to 1 = 37, which, of course, is the equivalent or proportional number of *muriatic acid gas.*

To effect its decomposition, 40 of potassium are required for 37 of muriatic acid gas, which 40 combine with 36 of chlorine, producing 76 of chloride of potassium, and liberating 1 of hydrogen.

I mentioned just now, that the muriatic acid of commerce, or aqueous solution of muriatic acid, was prepared by the decomposition of compounds containing it; and the process, although conducted on the large scale, is somewhat similar in its nature to that already described in the experiment for collecting the dry gas.

Common salt being a *chloride of sodium*, 32 parts of it are mixed, in a large retort, with 22 of sulphuric acid, diluted with one-third its weight of water; a receiver containing twice the weight of water employed in diluting the acid is now luted on the retort, which, being properly placed in a sand heat, distillation is proceeded with. The evolution of muriatic acid gas is not so intense and sudden as in your small retort, and there being abundance of water present, it absorbs the gas, so that after the process is completed, the aqueous solution in the receiver constitutes muriatic acid, as generally known. You must not confound this *aqueous solution* with *liquid muriatic acid*, for this means the gas evolved under great pressure, perfectly anhydrous, and which condenses into the liquid state; it is, however, rather a dangerous experiment, and, therefore, we will not meddle with it at present.

If you take some of the muriatic acid of the shops, pour it on to some granulated zinc in the gas bottle, (p. 363, vol. ii.) you will instantly find a copious evolution of *hydrogen*, the source of which is the *muriatic acid*, whose *chlorine* at the same time unites to the *zinc*, forming *chloride of zinc*, and this dissolves in the water, which undergoes no decomposition.

You may also effect its decomposition very beautifully by voltaic electricity, by the following arrangement.

Take a small wine glass or tube, fill it with *muriatic acid*, and tinge it *blue* with a little solution of indigo, made by adding some powdered *indigo* to *sulphuric acid* in a glass flask, applying a gentle heat; the indigo dissolves, forming an intensely *blue* solution, a single drop of which will colour a quart of water; then immerse two platinum wires, one at each side of the glass, and connect these with the voltaic battery. You will soon have evidence of the decomposition of the acid, by the abundant evolution of *hydrogen* at the *cathode*, and *chlorine* will at the same time be liberated at the *anode*, in sufficient quantity to *bleach* the whole of the blue colouring matter.

The foregoing are some of the most remarkable experiments concerning the composition and decomposition of *water* and *muriatic acid*, two most important compounds, and I do not think it necessary to pursue the subject further at present; other opportunities will frequently occur of again reverting to the subject, and of affording illustrations equally instructive and easy of performance.

QUESTIONS FOR SOLUTION RELATING TO METEOROLOGY HYDROGRAPHY, AND THE ART OF NAVIGATION.

By M. Arago.

[Concluded from p. 479, vol. II.]

PHENOMENA OF THE SEA.

Temperature of Currents.—Every one is acquainted with the works of Franklin, Blagden, Jonathan Williams, M. von Humboldt, and Captain Sabine, on the Gulf-Stream. No one now doubts that this Gulf-Stream is an equinoctial current, which, after having made the circuit of the Gulf of Mexico, and issued from the Straits of Bahama, moves from south-west to north-east, at a certain distance from the coast of the United States, retaining all the time, like a river of warm water, a greater or less degree of the temperature it had acquired between the tropics. This current divides into two branches. One of these, it is said, ameliorates the climates of Ireland, the Orkneys, the Shetland Islands, and Norway; the other, gradually bending, returns to its former path, by crossing the Atlantic *from north to south*, generally to the west of the Azores, but sometimes at no great distance from the coasts of Spain and Portugal. After a very long circuit, the waters of this branch rejoin the equinoctial current from which they separated.

Along the coast of America, the position, breadth, and temperature of the Gulf-Stream have been so well determined under each latitude, that a work has been published, without any appearance of quackery, under the title of *Thermometrical Navigation*, for the use of seamen in these latitudes. It is very desirable that the returning branch should be known with equal certainty. Its excess of temperature is nearly lost when it reaches the parallel of Gibraltar, and it is only by the assistance of the means of great numbers of observations, that any hope of finding it again can be entertained. The officers of the *Bonite* would greatly facilitate this research, if, from the meridian of Cadiz to that of the most western of the Canaries, they could determine the temperature of the ocean *every half-hour*, to the tenth of a degree.

We have spoken of a tepid current, but our navigators will, on the other hand, meet with a current of *cold* water along the coasts of Chili and Peru. This current, after leaving the parallel of Chiloé, runs rapidly from south to north, and conveys, as far as the parallel of Cape Blanc, the refrigerated waters of the regions near to the southern pole. The temperature of this current was first noticed by M. von Humboldt, and was subsequently observed with very great care during the voyage of the *Coquille*. The frequent observations on the temperature of the ocean which the officers of the *Bonite* will not fail to make between Cape Horn and the equator, will serve to correct, to extend, or to complete the important results obtained by their predecessors, especially by Captain Duperrey.

Major Rennell has described, with minute attention, the current which emanates from the south-east coast of Africa, and runs along the bank of the Agulhas. According to the observations of Mr. John Davy, the temperature of this current is 7°...9° Fahr. higher than that of the neighbouring seas. This high temperature is more deserving of the attention of navigators, from its being supposed to be the immediate cause of the cloud of vapour called the *Table-cloth*, which always envelops the summit of the Table Mountain, whenever the wind blows from the south-east.

TEMPERATURE OF THE SEA AT GREAT DEPTHS.—It is not to be expected that a vessel such as the *Bonite*, despatched on a mission the express purpose of which is to convey some consular agents to the most distant parts of the globe, will ever suspend its progress to engage in physical experiment. At the same time, as hours, and even entire days, of dead calm may be anticipated by the navigator, particularly when he has to cross the line frequently, we conceive that this expedition will act wisely by providing thermometrographical and sounding apparatus, for the purpose of sinking instruments in safety to the greatest depths of the ocean. There is now very little doubt that the inferior cold waters of the equinoctial regions are conveyed thither by submarine currents from the polar zones; but even if a complete solution of this theoretical point had been obtained, it would be far from depriving the observations we now recommend of their interest. Who does not see, for example, that the depth at which the maximum of cold is found, (we will say more, that that and every other degree of temperature,) must depend, in every latitude, and that directly, on the total depth of the ocean,—if we are ever to hope that the latter quantity may be, some time or other, deduced from thermometrical soundings!

TEMPERATURE OF SHOALS.—Jonathan Williams remarked, that water is colder on shoals than in the open sea. M. von Humboldt and John Davy confirmed the discovery of the American observer. Sir Humphry Davy attributed this curious phenomenon, not to submarine currents, which, interrupted in their course, rise up along the precipitous sides of banks and glide to their surface, but to radiation. By means of radiation, especially when the sky is clear, the superior beds of the ocean ought certainly to be greatly cooled; but every degree of cold, except in the polar regions, where the temperature of the sea approaches to the freezing-point, occasions an increase of density, and a descending movement in the beds cooled. If we suppose an ocean without bottom, the beds in question would sink to a great distance from the surface, and could modify but slightly the temperature; but if there be a shoal, and the same causes operate, the cooled beds would accumulate, and their influence must then become very perceptible.

Whatever truth there may be in this explanation, every one will perceive how much the art of navigation is interested in verifying the fact announced by Jonathan Williams, and which some recent observations seem to contradict; how eagerly also would meteorologists receive comparative measurements of the temperature of superficial waters in the open sea and above shoals; and, in particular, how acceptable it would be to them to see determined, by means of the thermometrograph, the temperature of the bed of water which *rests immediately on the surface* of the shoals themselves.

HEIGHT OF WAVES.—The young officers of the *Bonite* will probably be greatly surprised if we assure them that none of their predecessors have fully answered the following questions: What is the greatest height of waves during tempests? What is their greatest transverse dimension? What is the rate of their progress?

Observers have generally been satisfied with forming an estimate of the height. But, in order to show how erroneous such estimates may be, and the influence which the imagination exercises in such matters, we may state, that navigators equally deserving of confidence, have some of them given sixteen feet as the greatest height of waves, others have stated it to be above a hundred. What science, therefore, now requires, is, not rough guesses, but actual measurements, of which it is possible to appreciate the correct numerical value.

These measurements, we are aware, are attended with great difficulties, but which, however, are not insurmountable; at all events, the question is of too great interest to permit of any efforts to be spared which may be necessary to solve it. We have no doubt that our young fellow-countrymen will, in reflecting on the subject, devise themselves some means for performing the operation which we require of their zeal: a few brief considerations may assist in guiding them.

Let us suppose that for a moment the waves of the ocean were petrified and immovable; what should be done in a vessel also stationary and placed in a trough of the waves, in order to measure the real height,—to determine the vertical distance, of the crest of the wave, above the bottom of the trough? An observer should gradually ascend the mast, and stop at the point where the horizontal visual line, proceeding from his eye, appeared a tangent to the crest in question; the vertical height of the eye above the line of floatation of the vessel, always situated by the hypothesis at the bottom of the trough, would be the height required. This very operation it would be necessary to attempt in the midst of all the confusion and agitation of a tempest.

In a vessel at rest, so long as the observer does not change his place, the elevation of his eye above the sea remains uniform, and can be very easily determined. In a vessel tossed by the waves, the rolling and pitching incline the masts sometimes to one side, sometimes to the other. The height of any point of them, the main-top, for example, varies incessantly, and the officer who may have taken his station there cannot ascertain the value of his vertical co-ordinate, unless assisted by a second person placed on the deck, whose duty would be to observe the movements of the mast. If this line could be ascertained, within a foot, for example, the problem would appear to us completely solved, particularly if the moments chosen for observation were those when the vessel was nearly in her natural position; now she is precisely so when at the bottom of the trough.

It now remains to discover the means of determining whether the visual line touching the summit of a wave be horizontal.

The crests of two contiguous waves are of the same height above the intermediate trough. A horizontal visual line from the eye of the observer, when the vessel is in the trough, I suppose to be directed to the summit of the approaching wave; if this line be prolonged on the opposite side, it will likewise touch the summit only of the wave already past. This last condition is necessary, and is sufficient to establish the horizontality of the first visual line. Now, with the instrument known by the name of the *Dip-sector*, having its ordinary circles provided with an additional mirror, there may be seen at the same time, in the same glass, and in the same part of the field, two images, situate at the horizon, one before, the other behind. The dip-sector, then, will show to an observer, who gradually ascends the mast, at what instant his eye arrives at the horizontal plane, which touches the crests of two neighbouring waves. This solves, precisely, the problem proposed.

We have supposed this observation to be made with all the precision that nautical instruments admit of. The operation will be more simple, and sometimes sufficiently exact, if the observer merely determine, with the naked eye, the greatest height to which he could ascend the mast, without perceiving, when the vessel is sunk in the trough, any other wave than the nearest to that which may be approaching or receding. In this way the observation is within the power of all persons, and may be made even during the most violent tempests, that is to say, in circumstances when the use of reflecting instruments would be attended with difficulties, and when, moreover, perhaps no one but a sailor could venture with impunity to climb the mast.

The transverse dimension of a wave is easily determined, by comparing it with the length of the vessel as she passes through it. Its velocity may be measured by means well known. We have, therefore, in concluding this part, only to point out these two subjects of inquiry to the attention of the commander of the *Bonite*.

VISIBILITY OF SHOALS.—The bottom of the sea, *at a given distance from a vessel*, is more distinctly seen in proportion as the observer is elevated above the surface of the water: thus, when an experienced captain navigates a sea unknown, and abounding with shoals, he sometimes places himself near the summit of the mast, in order that he may direct his vessel with greater security.

The fact appears to us so well established, that we have nothing to request of our young navigators which relates to it in a practical point of view; but, by following the indications which we shall here point out, they may perhaps ascertain the cause of a phenomenon which affects them so nearly, and thence deduce more satisfactory means than casual observation has hitherto taught them to employ, for the purpose of detecting the position of shoals.

When a pencil of light falls on a diaphanous surface, whatever may be its nature, one portion of the light passes through, and another part is reflected. The latter is intense in proportion to the smallness of the angle formed by the incident ray with the surface. This photometrical law is not less applicable to rays which emanate from a rare medium, and meet the surface of a dense body, than to those which, *moving in a dense* body, strike the surface of separation of that body, *and of a rare contiguous* medium.

This being the case, let us suppose that an observer on ship-board wishes to perceive a shoal which is at a little distance,—for example, a submarine shoal, situate at 100 feet of horizontal distance. If his eye be about a yard above the sea, the visual line by which the light emanating from the shoal can reach it after issuing from the water, will form a very small angle with the surface of the fluid; if his eye, on the contrary, be very much elevated, suppose 100 feet, he will see the shoal under an angle of 45°. Now, the interior angle of incidence, corresponding to the small angle of emergence, is evidently less open than that which corresponds to the emergence of 45°. Under small angles, as has been seen, the strongest reflections take place; the portion of light which emanates from the shoal, and is received by the observer, will therefore be greater, the higher he is placed.

The rays emanating from the submarine shoal are not the only ones that enter the eye of the observer. In the same direction, and confounded with them, proceed rays of atmospheric light, which are reflected exteriorly from the surface of the sea. If the latter were sixty times more intense than those of the former, they would totally conceal the effect of them. The shoal would not even be suspected, for it has been proved by the experiments of Bouguer, and which have often been repeated since, that the most experienced eye is not sensible of an augmentation of light of $\frac{1}{60}$. If there be a less proportion between these two lights, the appearance of the shoal may not be entirely lost, but it will be very feeble. When it is remembered that atmospheric rays reflected to the eye from the sea, have a degree of splendour, which is greater in proportion as the angle under which they are reflected is acute, every one will perceive that two different causes concur to render a submarine object less and less apparent in proportion as the visual line approaches the surface of the sea,—namely, on the one hand, the progressive and real weakness of the rays which emanate from the object, and form its image in the eye; and, on the other, a rapid augmentation in the intensity of the light reflected from the exterior surface of the waters, or rather, if I may be allowed the expression, in the luminous curtain through which the rays issuing from the shoal must transmit their light.

On the supposition that the comparative intensities of the two superposed pencils are, as every thing leads us to believe, the only cause of the phenomenon which we are now analyzing, we have it in our power to point out to the officers of the *Bonite*, a better and far more easy means of detecting submarine shoals, than has been enjoyed by their predecessors. This means is very simple; it consists of looking at the sea, not with the naked eye, but through a plate of tourmaline cut parallel to the edges of the prism, and placed before the pupil of the eye in a certain position. A few words will render evident the mode in which this crystalline plate acts.

Let us assume that the visual line is inclined to the surface of the sea at an angle of 37°. The light which is reflected from the exterior surface of the sea under this angle, will be completely polarized. Polarized light, as every *physicien* knows, does not pass through plates of tourmaline suitably placed. A tourmaline, then, may eliminate entirely rays reflected by the water, which, mingling, in the direction of the visual line, with the light emanating from the shoal, either obstruct the view of it entirely, or at least greatly weaken it. When either of these latter effects is produced, an eye placed behind the plate will receive one kind only of rays, viz. those which emanate from submarine objects; and instead of two superposed images, a single image only will be formed on the retina; the visibility of the object which this image represents will thus be greatly facilitated.

The *entire* and *absolute* elimination of the light reflected from the surface of the water, is only possible under an angle of 37°, because it is under this angle alone that it is *completely polarized;* but under angles from 10° to 12° greater or less than 37°, the number of polarized rays which the tourmaline can arrest in the reflected light is still so considerable, that the use of the same means of observation cannot fail to be attended with very advantageous results.

By engaging in the trials which we now propose to them, the officers of the *Bonite* may throw light on a curious question of photometry; they may probably confer on navigation a means of observation which may prevent many shipwrecks; and by introducing *polarization* into the nautical art, they may furnish a new instance of what those individuals expose themselves to, who unceasingly receive experiments and theories which may have no present practical application with a contemptuous *cui bono?*

WATER-SPOUTS.—Has electricity any influence in producing water-spouts? A distinct and indisputable answer to this question would possess great interest. The officers of the *Bonite* ought therefore to exert themselves to discover, whenever this phenomenon presents itself, if it produce thunder and lightning.

DEPRESSIONS OF THE HORIZON.—The distinctly-defined blue line, forming the apparent separation between the sky and the sea, to which sailors refer the position of the stars, is not in the mathematical horizon; but the distance at which it appears below the latter, and which is called *the depression*, may be calculated exactly, since it depends merely on the height of the observer's eye above the sea, and the dimensions of the earth. It is unfortunately not so easy to appreciate the effects of atmospheric refraction. It must even be stated, that in the calculations of the tables of depression usually employed, the *mean refraction* relative to a certain state of the thermometer and barometer, is the only one taken into account. Officers of great skill, Captain Basil Hall, Captain Parry, and Captain Gauttier, have determined, by observations, the errors to which navigators are exposed by following the common rule. They measured, either with the *dip-sector* of Wollaston, or with ordinary instruments furnished with an additional mirror, and in the most varied states of the atmosphere, the angular distance of one point of the horizon from another diametrically opposite. Admitting, as is generally done, that the state of the air and of the sea are the same all around the observer, the difference of the distance measured, and of 180°, is evidently double the real depression of the horizon. The half of this difference, compared with the depression of the tables, gives, therefore, the possible error of every angular observation of altitude made at sea.

The positive and negative errors observed by Captain Parry in the northern regions, were all comprised between + 59″ and − 33″. In the Chinese and Indian Seas, Captain Hall found greater deviations, viz., from + 1′ 2″ to − 2′ 58″. Finally, Captain Gautier, in the Mediterranean and Black Seas, observed still greater differences, viz., from + 3′ 35″ to − 1′ 49″. If it be recollected that the variation of a single minute in latitude corresponds to a deviation of above 2100 yards on the globe, it will be universally acknowledged how deserving of attention is the investigation which we have mentioned.

By examining with care all the observations of MM. Gauttier, Basil Hall, and Parry, a conclusion is attained, *that the error of the* CALCULATED *depression is not* POSITIVE, *and that this depression does not exceed that which is observed,* EXCEPT *in the degree that the temperature of the air is higher than the temperature of the water.*

With regard to the *negative errors*, they present themselves indiscriminately in all the comparative thermometrical states of the sea and atmosphere, without the possibility of attributing these anomalies to any apparent cause, and, in particular, to the state of the hygrometer. Here is, therefore, a very curious problem for solution, and which is equally interesting to the *physicien* and the seaman.

MISCELLANEOUS OBSERVATIONS.

RISING-UP OF THE COAST OF CHILI.—In the month of November, 1822, after the earthquake which overturned the towns of Valparaiso, Quillota, &c., in Chili, great part of the country was found to be elevated from one to two yards above its former level. The earthquakes of 1834 appear to have been still more violent than that of 1822. It would therefore be important to examine whether they did not, like the latter, produce an instantaneous elevation of the country. A beach on which the sea, by the effect of the tides, never rises above one or two yards, might furnish a multitude of opportunities, such as landing-places, oyster-banks, and muscles and other shell-fish adhering to rocks, by means of which all doubts on the subject might be removed. A glance at the localities will do more in this respect, than the necessarily vague indications which we can furnish here. We believe, however, that we ought to mention *the lake of Quintero, which communicates with the sea*, as well fitted to afford indisputable proofs of changes of level. Recourse should likewise be had to the hydrographical charts of Vancouver, Malaspina, &c., for it is by no means probable that these risings should be confined to the shore, and that the bed of the sea should not participate in them.

Abrupt or gradual elevations of the ground appear destined to perform too important a part in the history of the earth, for us to omit to invite most particularly the officers of the *Bonite* to take note of all recent phenomena of this nature which they can possibly discover, and in an especial manner to direct their attention to the coast of Peru*.

EARTHQUAKES.—According to an opinion very generally entertained in America, earthquakes are more frequent in certain seasons than in others. If this were fully ascertained to be the fact, it would be of extreme importance in the physics of the earth. A complete collection of the journals which have been published in Chili for the last twenty years, made with regard to this point, would certainly throw some light on the question. We recommend this object to the commander of the expedition, either that he may perform the task during the voyage, or be content with collecting materials for it.

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* Captain Fitzroy, on the recent trial of Captain Seymour of the Challenger, stated, that in the earthquake of Feb. 1835, the island of Sta. Maria had risen ten feet.

To adopt popular opinions without the slightest hesitation, is to run the risk of introducing into science, to its great injury, a multitude of confused notions founded on phenomena imperfectly seen and inaccurately examined; but to reject such opinions without examination, is often to lose an opportunity of important discovery. From this consideration I do not hesitate to entreat our young countrymen to inquire, during their occasional stoppages on the western coast of America, whether the phenomena which are said to have attended the earthquake which destroyed Arica and Saena on the morning of the 18th September, 1833, have been observed in any other places. The following is an account of them by Mr. John Reid, an English traveller.

"The continual baying of dogs and braying of asses announced the approach of danger. On the preceding day the atmosphere had been of an alarming stillness. With the exception of a few extraordinary gusts, coming sometimes from one quarter and sometimes from another, *and which were felt quite as well in the interior of an apartment as on the outside*, it might be said that during the whole of the day of September 18th, the air at Saena was completely stagnant.

"The shocks left a great number of empty bottles in the places which they occupied, but their corks were found scattered about in all directions. None of these empty bottles had been even overturned; others, on the contrary, that were filled, were thrown out of their places and broken. The varnish on a new table belonging to Mr. Reid became so fluid, that the day after the earthquake the mahogany appeared surrounded with ropy glue. Some large jars of earthenware which had been sunk in the earth and contained water, had the water thrown out of them on the surrounding ground, although the surface of the water was kept *three or four feet* below the mouths of the jars.

"At Saena it was remarked, that after a shock, whether slight or severe, all the dogs of the town went to quench their thirst at the first pool of water they could fall in with."

ELEMENTARY COURSE OF DESCRIPTIVE GEOMETRY.

III.

GENERAL REMARKS ON METHOD AND PRINCIPLES.

IN conducting investigations to *a theoretical conclusion*, the chief skill consists in fixing upon the best relations or positions of the hypothesis with respect to the co-ordinate planes; but in reference to a practical construction, the data must be taken in the most unconfined manner that they admit of, and the skill consists in the most general mode of expression or delineation. It is to the construction of problems that the Descriptive Geometry more especially applies; the demonstration of theorems is not often materially facilitated by its employment,—at least in its primary character. A great portion of the subjects which some writers (Monge amongst them) include under this title, would require a more extended definition than has yet been given of the science. Where a figure is not considered by means of its horizontal and vertical projections, it does not appear to us to belong *essentially* to the Descriptive Geometry, but rather to the *Rational Geometry* of space generally, independent of any particular mode of projectional representation. It is true, that in considering a *problem* in the Descriptive Geometry, we may be led to many *theorems* of importance respecting the data of our problem; and if the demonstrations naturally arise out of the considerations which belong to the problem as a descriptive one, they ought to be considered as belonging to that branch of the science; but if their demonstration is more readily effected by means of the considerations arising from the relation between the data in the figure itself, we ought to refer it to the *Rational Geometry of Space*. Very often, too, in studying the best mode of representation of a problem in Descriptive Geometry, it will happen that an important theorem concerning the represented figure will present itself; and this ought, so at least it appears to us, rather to be placed amongst the theorems of Rational Geometry, than in the Descriptive part of the science. When, however, the theorem and its demonstration naturally arise as a consequence of the lines employed in the projections, then, and then only, the theorem properly belongs to the Descriptive Geometry*.

Another very important point should be kept in view by the reader of these papers. Models, however rudely constructed, are indispensable, in order to his fully comprehending the character of the system, and the force of its demonstrations. This is alike the case in the study of the Rational and Descriptive Geometry. We have always found that when models were employed, the propositions relative to the intersections of lines and planes, to dihedral and solid angles, and to the properties of lines and planes variously conceivable in solids, generally became quite as intelligible to the student as the corresponding propositions in plane geometry, and even more readily acquired than these were. No tutor should, from indolence or any other cause, forego the employment of them; and no student who regards either facility of acquisition or clearness of perception, in this study, can dispense with them.

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* We had drawn up a series of propositions on *Rational Geometry*, with a view of extending this class of researches to a degree not before done by any writer on the Geometry of three dimensions: but even after putting nearly a sheet of them in type, we have been led to relinquish the design, under the impression that they were not properly adapted to our Magazine. They will, however, appear in another place, to which, when they are published, we shall refer our readers.

It must be remembered, that in the Rational Geometry we reason concerning the things themselves which are the subjects of the proposition. In plane geometry, we can give a more or less accurate representation of them upon paper; but when all the subjects of the hypothesis are not in one plane, we can only represent them *in plano* by means of some kind of projection. The mind is bewildered by the continued claim upon the attention which is required, for discriminating between the visual object and the thing represented by it. Lines are traced upon one plane to represent lines not so traced; angles which are, in fact, right ones, are projected on the picture into acute or obtuse ones; lines which are parallel in the hypothesis, are pictured (if the perspective be accurate) converging to a point; lines which are neither in the same plane nor parallel to one another, in some cases, may be represented by parallel ones in the picture; a great number of angular points, only a certain number of which can, by the hypothesis, be in one plane, are all projected upon a single plane;—and a hundred other difficulties of the same kind occur. Can we wonder, then, if we almost invariably find a vagueness in the conceptions, and a total absence of all mental vigour in grappling with questions relating to space, when we reflect that the student is left to form his conclusions from such confused and contradictory representations as the usual figures (always incorrectly drawn, too,) are calculated to furnish. A feeling of disgust with the study itself is the inevitable consequence, and the entire abandonment of the subject by students in general is a complete proof of our assertion. Let the student, however, adopt the practice of making models of his figures relative to space; and then the simplicity and elegance of the rational processes will be so striking as to insensibly create a devotion to it, which forms a strong contrast with the repulsive feelings with which he had before approached to it. To tutors we would say,—You are false to the trust reposed in you, if you do not adopt this plan in your professional duties.

Again, with respect to models in the study of Descriptive Geometry, we would urge with equal earnestness its adoption in this case, too, or, if possible, with greater earnestness. It must be recollected that the *demonstration* of the accuracy of the assigned operations depends entirely upon reasonings respecting lines and planes variously drawn with respect to the planes of projection. The properties of the projections and traces themselves are derived entirely from such reasonings; and he that is not aware of this, or sufficiently impressed with this, has little chance of obtaining a firm hold upon the methods of the science, or power to investigate for himself either a property or a method of construction which will be of any essential use to him, and upon which he can place any satisfactory dependence.

Moreover, the *interpretation* of a Descriptive Projection, or the projectional representation of a figure, can never become familiar operations, except we adopt this practice; and without this power being very familiar, it is impossible to render Descriptive Geometry subservient to ulterior objects, whether mathematical, physical, or practical. We are quite convinced that the great success which attended the professorial labours of Monge and Hachette in the Polytechnic School was due quite as much to their employment of models as to their elegant developement of the principles, to the enforcement of which those models were rendered subservient. The models in that celebrated school were made by the hands of Hachette himself; and so much pains did he bestow upon them, that he laughingly to a friend of ours called them "his children." Of course we do not wish our readers to adopt the idea that silver is the necessary material of which to construct them, or that they require to be made with the same nicety that an instrument for delicate physical experiment is made, because Hachette made many of his of that material, and employed so much care upon their construction. We should as soon think of recommending all prescribed geometrical constructions to be performed with silver compasses and platinum scales; but we do urge that if geometrical investigations require a representative figure to be drawn with some degree of care, there is infinitely more necessity for some kind of model in the study of a proposition referring either to Descriptive Geometry or the Rational Geometry of Space.

We have often wondered that our instrument-makers have never ventured to undertake a series of models adapted to the eleventh and twelfth books of Euclid; especially as this is the professed text-book in our universities and schools. The French have such a set adapted to Legendre's Geometry, which are very compactly packed in a small case, considerably less than our common portable writing-desks. We feel it but just to confess, that though the Geometry of Three Dimensions had been long a favourite study, yet the contemplation of those elegant models was to us of great personal service in enabling the mind to grasp the *tout ensemble* of the data or hypothesis, and the auxiliary lines and planes which were employed in the investigation. We would, therefore, urge all who are concerned either in teaching or in the study of this branch of science, to form, as well as they can, such models for themselves *pari passu*. Most of them may be readily made of card-board, silk-threads, and brass-wire, with sufficient precision to answer all the processes for which they are constructed.

One remark more, respecting the form in which the following propositions are developed, may be necessary here.

It is familiar to every one who has looked at geometry as a science, that the ancient mathematicians not only demonstrated the accuracy of their method of constructing a problem, but that they had also a method of *analyzing* it, as a preparatory step to their obtaining the construction itself; from which analysis the method of construction was actually deduced. This analysis was a secret art amongst them, and few traces of it are to be found in their writings. Indeed, except from that valuable relic of the ancient geometry, the mathematical collections of Pappus, we have no *direct* evidence of their knowledge of this art; though it would be abundantly evident, from other considerations, that such must have been the case, even had the fragments of

Pappus perished in the dark ages, when so many of the ancient writings were totally and irretrievably lost[*].

So far as the analysis of a problem is concerned, it consists in supposing the problem actually constructed, and in then examining the relations which subsist between the *data* and *quæsita*. This is generally, in principle, the course pursued by writers on the Descriptive Geometry; and hence a brief statement of its nature becomes necessary, so far as it is employed here.

The essential character of the Descriptive Geometry as a science is that its *reasonings* are concerning the figures situated in *space*, whilst all its *constructions* are executed *in plano*.

In the outset, the lines and the planes, as well as the planes of projection, are supposed to be in their own proper positions, and lines and planes are variously conceived to be drawn in specified directions and positions; and the reasoning is concerning the consequences of these constructions, till at length the figures made by the traces of the several planes and surfaces upon the planes of projection are completely determined. The constructions derived from these reasonings, to be effective, are always such as can be made either wholly upon one of these planes or wholly on the other, generally upon either of them taken at pleasure. In the investigation, therefore, the data are all supposed to be in their respective natural places; in the construction, the planes of projection are supposed to be brought into the same plane by the revolution of one of them about the ground-line, carrying with it all the traces and projections of the several data of the problem ; and the constructions which the investigation or analysis (we prefer the former term, since the analysis, in its usual acceptation, is not rendered formally complete,) has pointed out, are performed in this plane. No formal demonstration is generally necessary, as it is almost always a mere obvious reversal of the investigation; and this, too, is in accordance with what takes place in the regular analysis of a problem. Where it appears to be necessary, we shall give a demonstration, but not otherwise.

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[*] To those who are not well versed in the philosophy of geometrical reasoning, we recommend an attentive study of the third book of the *Mathematical Collections* of Pappus, the notes appended to Dr. Traill's *Life of Simson*, and Fryer's edition of Lawson on the *Geometrical Analysis of the Ancients*. With respect to examples, as Dr. Traill many years ago recommended to us Leslie's *Geometrical Analysis*, subsequently published as the first part of his second volume of *Geometry*,—and though there are occasional errors in it, we know no one work better adapted to create a good taste in this department of Geometry. Wildbore's demonstration of Lawson's Theorems in the *Manchester Transactions*, or in the sixth volume of Leybourn's *Mathematical Repository*, is also an elegant and instructive series of examples of the mode of successive derivation of theorems, though it can hardly be called, as some have called it, a formal system of analysis. Swale's *Geometrical Amusements* is a work which we cannot too earnestly recommend to the young student, as the best series of examples with which we are acquainted: but this, we believe, like Fryer's Lawson, is only to be met with occasionally; and we confess we are surprised that neither of them have been reprinted. The great storehouse, however, of geometrical analysis is, the periodical mathematical literature of the last, and early part of the present century: and, especially, Leybourn's *Mathematical Repository* (both series), the *Ladies' and Gentlemen's Diaries*, and the *Mathematical Companion*. To these may be added, but they are, for the most part, extremely scarce, the *Liverpool Student*, the *Hull Quarterly Visitor*, the *Boston Enquirer*, and the *Leeds Correspondent*, Turner's *Mathematical Exercises*, the *Mathematician*, Burrow's *Diaries*, Swale's *Liverpool Apollonius*, Whiting's *Scientific Receptacle*, and *Mathematical Delights*, the *Stockton Bee*, Heath's *Palladium*, Martin's *Magazine*, as well as many of the older volumes of the *Gentleman's Magazine*, the *Town and Country Magazine*, and several of the other series of monthly works. Many excellent papers on Geometry are also interspersed through the *Philosophical Magazine*, the *Annals of Philosophy*, Nicholson's *Journal*, the *Quarterly Journal of the Royal Institution*, and Brewster's and Jamieson's *Edinburgh Philosophical Journals*. Of all those works, as well as several not here enumerated, and of the *Continental* journals generally, we shall speak more at large in another place, and on another occasion. Our present object is merely to direct the young student to the places in which he may find elegant models of the Geometrical Analysis, from which to form his taste and his style.

IV.

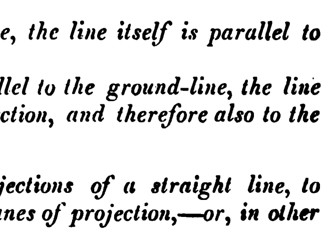
PROPOSITIONS CONCERNING THE STRAIGHT LINE AND PLANE.

PROP. I.—PROB. Given the projections of two points upon the plane of the picture, to describe the projection of the straight line passing through them.

Draw the straight lines through the projections upon the planes of projection, these will be the projections of the line sought, as is evident from page 308.

PROP. II.—PROB. Given the projections of a line, and a point in one of the projections, to find the corresponding point in the other projection.

Let X'X be the ground-line, R'S' and R''S'' the horizontal and vertical projections of RS, and P' a point in the projection on the horizontal plane. Draw P'P'' perpendicular to X'X, meeting R''S'' in P'. This is the vertical projection of P. (See Art. 17.)

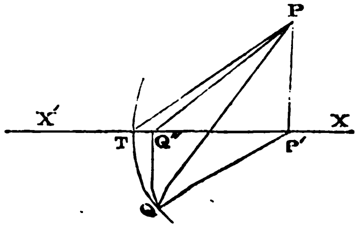


In a similar manner, had P'' been given P' might have been found.

Cor. 1.—*If one of the projections upon plane be parallel to the ground-line, the line itself is parallel to the other plane of projection.*

Cor. 2.—*If both projections be parallel to the ground-line, the line itself is parallel to both the planes of projection, and therefore also to the ground-line itself.*

PROP. III.—PROB. Given two projections of a straight line, to find the intersections of the line with the planes of projection,—or, in other words, to find its traces.



Invest. Let P and Q denote the points in which it meets the plane. Let the vertical projecting planes cut the planes of projection in QP' and P'P. Then since the projecting plane QPP', and the vertical plane of projection are both perpendicular to the horizontal plane, their intersection PP′ is perpendicular to it (*Course*, theor. 110); and hence to the ground-line X′X in that plane.

In the same manner, the line QQ″ is perpendicular to X′X.

Constr. Hence produce the given projections of the line to meet the ground-line in P′ and Q″ respectively; from P′ and Q″ draw lines perpendicular to the ground-line, meeting those projections in P and Q respectively. Then these are the points in which the line whose projections are given cut the plane.

Prop. IV.—Prob. To find the angles which a line whose projections are given makes with the planes of projection.

Invest. Let P and Q be its intersections with the planes of projection, and let QPP′ be its vertical projecting plane. Then since the plane PQP′ is perpendicular to the horizontal plane, PQP′ is the angle formed by the line PQ and that plane, and PP′Q is a right angle. Let the plane PQP′ revolve about PP′ till it coincides with the vertical plane of projection; then, since PP′Q is a right angle, the line P′Q will always be in the horizontal plane, and the point Q will describe a circle in it cutting the ground-line in T; and in that position of the plane PQ will coincide with PT, and be equal to it.

Constr. Find the traces of the line P and Q (prop. iii.); and then with centre P′ and distance P′Q describe a circle cutting the ground-line (on either side of P′) in T. Join TP, then the angle PTP′ is equal to the angle made by the line and the horizontal plane of projection.

A similar construction, with an interchange of the planes of projection for each of the operations, will give the angle formed between the line whose projections are given, and the vertical plane of projection.

Prop. V.—Prob. Given the projections of two straight lines, to find the angle comprised between them.

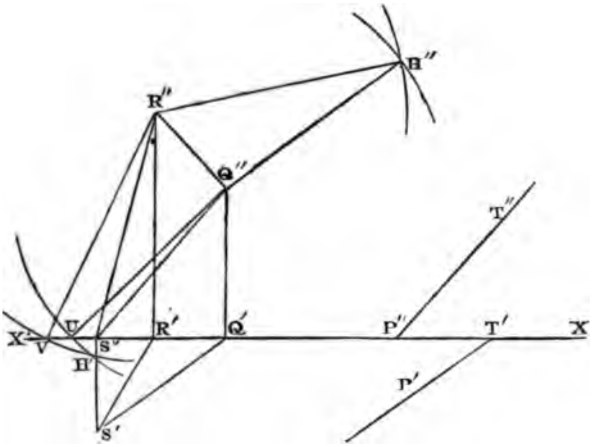
[When two lines, AB, A′B′ are not in the same plane, the usual definition of an angle does not apply to their mutual inclination. It may, however, be easily shown that two planes, AH and HA′B′, can be drawn one through each line, at right angles to each other. The angle formed by the line A′H, drawn parallel to one of the lines AB, and the other line A′B′, or the angle HA′B′, is called the inclination of the lines AB, A′B′ to one another, or the angle contained between the lines AB, A′B′.

This more general definition evidently includes that of an angle *in plano.*]

From the remarks in the brackets, it is obvious that the problem is reduced to finding the angle contained by two lines in the same plane, one of which is parallel to one of the given lines. Let the line drawn parallel to one of the given lines pass through the horizontal trace of the other.

Invest. Thus, let T″P′ be the traces of one of the given lines, and R″S′ those of the other. Through S′ draw S′Q′ parallel to P′T′, and through S″ draw S″Q″ parallel to P″T″, and Q′Q″ perpendicular to the ground-line, meeting it in Q″.

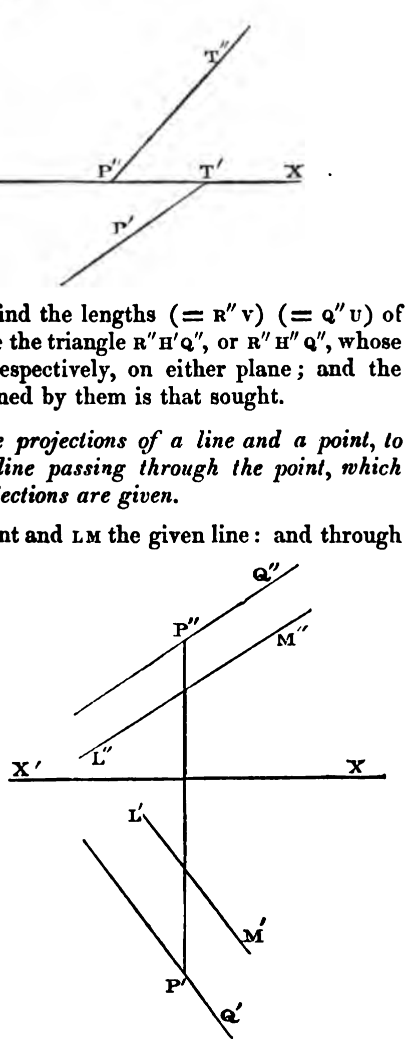
Then, (prop. iii.) the points S', Q'' are the horizontal and vertical traces of a line parallel to $T''P'$ and passing through S'. Join $R''S'$ and $Q''S'$; then the three sides of the triangle $R''S'Q'$ are known.



Constr. Hence by prop. vi. find the lengths ($= R''V$) ($= Q''U$) of $R''S'$ and $Q''S'$, and on $R''Q''$ describe the triangle $R''H'Q''$, or $R''H''Q''$, whose sides are equal to $R''S'$ and $Q''S'$, respectively, on either plane; and the angle ($R''H'Q''$, or $R''H''Q''$,) contained by them is that sought.

Prop. VI.—Prob. *Given the projections of a line and a point, to draw the projections of a second line passing through the point, which shall be parallel to that whose projections are given.*

Invest. Let P be the given point and LM the given line: and through P conceive the line PQ to be drawn: then through PQ draw projecting planes parallel to the projecting planes of LM: then, since the projecting planes through the parallel lines PQ and LM are both perpendicular to the planes of projection, they are parallel, and their intersections with these planes are also parallel. That is, $P'Q'$ is parallel to $L'M'$ and $P''Q''$ to $L''M''$. Moreover, P is in the line PQ, and hence its projection P' is in the projection $P'Q'$: and in the same manner the other projection of P, viz. P'', is in the projection $P''Q''$.



Constr. Through the given projections of the point draw lines parallel to the given projections of the line. These are the projections sought.

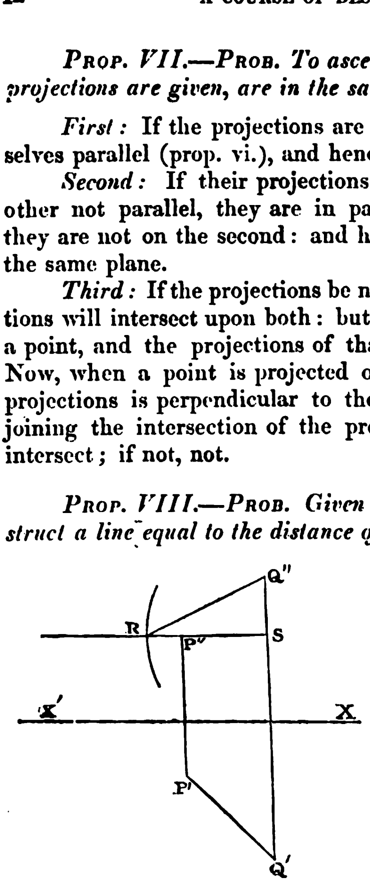
Prop. VII.—Prob. *To ascertain whether two straight lines, whose projections are given, are in the same plane or not.*

First: If the projections are in both cases parallel, they are themselves parallel (prop. vi.), and hence they are in one plane.

Second: If their projections on one plane be parallel, and on the other not parallel, they are in parallel planes on the first account, and they are not on the second: and hence they are neither parallel, nor in the same plane.

Third: If the projections be not parallel in either plane, those projections will intersect upon both: but if the lines intersect, they intersect in a point, and the projections of that intersection will be that of a point. Now, when a point is projected on two planes, the line joining those projections is perpendicular to the ground-line. If, therefore, the line joining the intersection of the projections fulfil the condition, the lines intersect; if not, not.

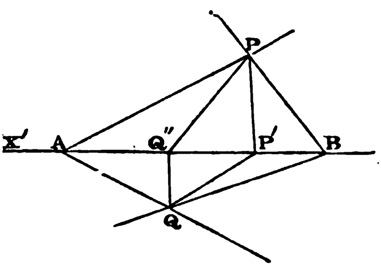
Prop. VIII.—Prob. *Given the projections of two points, to construct a line equal to the distance of those points.*



Let P, Q be the given points, and P′, Q′ and P″, Q″ their projections. Through P draw a plane parallel to the horizontal plane, cutting the vertical plane in RS. Then the line PQ is the hypothenuse of a right-angled triangle, whose two sides are P′Q′ and Q″S′.

Constr. Through either of the extremities of either of the projections, as P″, draw a line, RS, parallel to the ground-line, meeting the projecting parallels of the other extremity, P′P″ in S. In SP″ produced take SR = P′Q′ and join RQ″. This is the length of PQ.

Prop. IX.—Prob. *Given the traces of two planes, to find the projections of their line of intersection.*

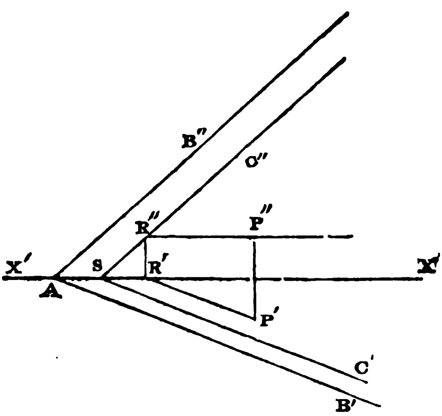


Invest.—Let A P, A Q, be the traces of the one plane, and B P, B Q, those of the other: then P Q is their line of intersection. Then since the intersection of the traces is a point common to both planes, it is a point in their common intersection (the traces of the planes being in the planes themselves): hence P and Q are points in the common intersection of the plane itself.

Constr.—Find (by prob. iii.) the projections, P′Q and P Q′, of the line PQ, joining the intersections of the traces upon each plane of projection. These are the projections required.

Prop. X.—Prob. Given the traces of a plane, and the projections of a point, to draw the traces of a plane passing through the given point, and parallel to the given plane.

Invest.—Let A B′, A B″, be the traces of the plane, and P′, P″, the projections of the point. If now a line be drawn from P, parallel to the intersection of the given plane with the horizonal plane (that is, the horizontal trace), it will be parallel both to the given plane and to the horizontal plane, and will therefore lie wholly in the plane sought. Then, also, since the line through P is horizontal, its vertical projection is parallel to the ground-line; and since it is parallel to the trace A B, its horizontal projection is also parallel to that trace (th. i.) The horizontal and vertical projections are therefore given, and hence the point R″, in which it cuts the vertical plane of projection, is also given. But since this point is also in the plane through P, parallel to B′ A B′, this plane also passes through R″. Also, since the planes are parallel, their traces are parallel (*Course*, theor. 108). Hence we have this



Constr. Through P′ draw P′R′ parallel to the trace A B′ of the given plane, and through P″ a line P′R″ parallel to the ground-line, meeting the perpendicular through R′ in R′. Through R″ draw C″R″A parallel to A B″, meeting the ground-line in S; and through S draw SC′ parallel to A B′. Then SC′ and SC″ are the traces required of a plane through P parallel to B′ A B″.

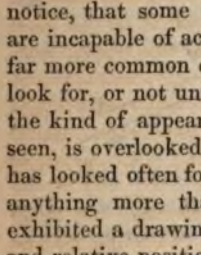
Instead of this construction, an interchange of the planes of projection, and of their corresponding operations, might have been employed, and the resulting traces would, evidently, have been the same.

We hope to be able to complete the problems respecting the straight line and plane in our ensuing paper.

DARK LINES IN THE PRISMATIC SPECTRUM.

In our first volume we offered a general sketch of the recent progress of optical researches, in the course of which we were necessarily led to speak of the existence of dark lines in the prismatic spectrum; a phenomenon first noticed by Dr. Wollaston, and afterwards more precisely examined by Fraunhofer; and with respect to which, much curiosity has been excited, and sometimes no little difficulty felt, as to the general practicability of verifying the observation, at least without great skill and very delicate apparatus. We purpose, therefore, in the present instance, to make a few remarks on the general nature of this phenomenon, and the means of observing it, for the satisfaction of those readers who may be interested in so curious an appearance, and may have experienced any of the difficulties alluded to in their attempts to witness it.

It seems not improbable, from facts which have fallen under our notice, that some individual eyes are so constituted, that they perhaps are incapable of accurately distinguishing these lines; but we imagine a far more common cause of failure is a want of a previous notion what to look for, or not unfrequently, a prepossession absolutely erroneous, as to the kind of appearance to be expected; so that the real object, though seen, is overlooked. We have known instances in which an individual has looked often for them with a very good apparatus, without perceiving anything more than the ordinary spectrum; and yet, when we have exhibited a drawing, representing, even roughly, the general appearance and relative position of the lines, the party has been able immediately to recognise them. Large and accurate engravings of the lines, as seen with a good telescope, are given in Sir J. Herschel's Treatise, and other works; but these give little assistance to a learner in detecting the phenomenon for the first time, from their being complicated by a vast number of lines, which, under ordinary circumstances, are not seen; whilst those which are ordinarily the most prominent and best marked in these representations, lose their distinctive characters, and appear no more conspicuous than the rest. These are, however, perfectly correct representations; for the character and appearance of the lines are more or less totally changed, according to the magnifying power employed.



When we look at the spectrum formed from the light of the clouds admitted through a well-defined narrow slit in a screen or window-shutter, by a moderately good prism of flint-glass, with the unassisted eye, the spectrum appears marked with about ten or twelve lines of different degrees of darkness and breadth, at irregular intervals, all parallel to the slit, in the successive portions of the coloured spectrum; the chief of these are represented in the annexed rough sketch.

The seven principal lines fixed upon by Fraunhofer for his measurements are marked by the letters B, C, D, &c., according to his nomenclature. The lines B and C can seldom be seen, unless the eye be guarded by a deep-blue glass; and the remarkable double line H (which cannot be mistaken when once seen,) requires a very bright sky, or a part of the clouds very near the sun's disk. They may all be seen most commodiously by throwing the direct rays of the sun into the room by a plane reflector. None of the lines form exactly the boundary between any of the coloured spaces. Much depends on the prism: the best flint-glass prisms, commonly met with at good opticians', show the principal lines well enough. Crown-glass prisms, though to the eye apparently quite as clear, do not show them at all: but prisms of liquids between inclined glass plates exhibit the lines with great perfection. Oil of aniseed may be tried with peculiarly good effect. The employment of a small achromatic telescope, through which the spectrum may be viewed, (in the manner of the apparatus sketched in our Number for July last,) materially assists in giving distinctness to the appearance, and brings many smaller lines into sight. When higher powers are employed, the appearance of the individual lines undergoes a change. Some of those which before were broad and conspicuous, are now resolved into a collection of small and faint lines, and thus these standard lines cease to be the most marked and defined points of the spectrum: they are in this respect analogous to double stars; and probably if higher powers were employed in these observations, we should go on resolving all the apparently single lines into combinations of others; so that the general appearance, under higher powers, would retain no resemblance to that seen with lower, or the naked eye.

In the article referred to, we have noticed the important use made of these lines as points of measurement in the determination of the refractive indices for different media: they are, however, objects of highly interesting inquiry in themselves; more especially as no satisfactory explanation of their nature or origin has yet been started. The provisional supposition is, that they are absolute interruptions in the continuity of the spectrum; or the vacant spaces left by certain definite rays absolutely deficient in the solar light. But it is quite conformable to the progress of inductive science to expect that this supposition may in time be analyzed into some combination of simpler elements, and traced to some higher or known principle. It has not escaped the surmises of some philosophers, that the great principle of *interference* may have some connexion with the results, though in the present state of our knowledge we cannot determine this point. There are many cases in which the nature of the medium is such that the lines cannot be seen. There is one resource which has been indeed specifically proposed and employed by Sir J. Herschel, and which, in many cases, is the only one of which we can avail ourselves; and this is the absorption of definite rays by certain coloured media. The common dark-blue or purple glass is highly valuable in this respect: it gives certain well-marked distinct characteristics of the spectrum (when viewed through it) at certain definite parts; and if these be well studied in conjunction with the more precisely defined dark lines, much may be done in many cases where the direct observation of those lines themselves is impracticable.

This is true to a still greater extent in respect to the later discovery made by Sir D. Brewster, of the peculiar absorption which an interposed medium of nitrous acid gas exercises on the solar spectrum. He has now distinctly made out that the dark lines which it produces are invariably no more than enlargements of lines previously existing in the simple spectrum, though the changes thus made in its appearance are so great as for a time to mislead the judgment, by the idea that an entirely new set of lines are formed.

If we recur to the measurement of the unequal refrangibility in different media, the importance of accurate examination of the dark lines will be manifest. It is by this means that all the later advances in the science have been effected. In its earlier stages the subject of dispersion made little progress. It was fettered with difficulties, which, in fact, arose wholly from an unaccountable adherence to an imaginary principle of uniformity to which it was supposed nature would be restricted: it was cramped by the limited and mechanical spirit in which the inquiry was pursued as connected only with practical objects; and an indirect and troublesome kind of observations was employed, which, after all, afforded no very exact results. But from the system introduced by Fraunhofer, these have now all given way to that union of directness with precision, of simplicity with comprehensiveness, in method and results, which usually mark the substantial progress of true experimental science, and which characterize investigations carried on in conformity to the real spirit of nature. Each of the simple components of light is designated and identifiable by means of the definite lines, and the direct observation of the angle through which any single ray is deviated after successive refraction at the two inclined surfaces of a prism of any medium, gives at once the index of refraction, or numerical expression of the ratio, for that ray, and that medium, which is the essential element in all calculations, whether for theoretical or practical purposes. In our report of the Bristol meeting of the British Association, we mentioned a series of observations of this kind laid before the physical section, for a variety of different media, by Professor Powell. These observations have since appeared as one of the tracts published by the Oxford Ashmolean Society, together with some preliminary remarks, containing an historical sketch of the progress of the inquiry. The theoretical bearings of these results are discussed by the same gentlemen, in a paper lately read before the Royal Society.

TRANSACTIONS OF THE ROYAL SOCIETY OF EDINBURGH.

Vol. XIII. Part I.

In conformity with our promise in a previous number, we proceed to give our readers an analysis of the contents of this part of the Edinburgh Transactions.

I. *On the Investigation of Magnetic Intensity by the Oscillations of the Horizontal Needle. By* W. Snow Harris, Esq., F.R.SS. London and Edinburgh.

The Royal Society of Edinburgh not publishing its Transactions at regular intervals, it happens generally that the contents of the earlier papers in each fasciculus become pretty generally known to the scientific world through other sources, before we obtain the paper in its complete form in the publications of the Society. Such is the case with the present paper.

Its object is to enforce the performance of experiments on the vibration of the magnetic pendulum in vacuo,—at least in the very attenuated state of the atmosphere which the air-pump enables us to obtain. He concludes, from his experiments, which are made with great care and considerable address, that "if the oscillations be taken *in vacuo*, the differences in the arcs of vibration in sunshine and in shade, may, under certain conditions, vanish, or nearly so; as also, that the time of a given number of vibrations is, upon the whole, in a void, rather increased than diminished."

The former of these conclusions, should it be found justified by subsequent experiments, (which we confess we greatly doubt will be ultimately the case,) will show that the magnetic intensity itself is not dependent for any modification whatever upon temperature. The latter is only what we should expect from the known laws of the pendulum.

II. *An Account of some Experiments on the Electricity of Tourmaline and other Minerals when exposed to Heat. By* James David Forbes, Esq., F.R.SS. Lond. and Ed., *Professor of Natural Philosophy in the University of Edinburgh.*

Professor Forbes having, during the summer of 1831, been much engaged in studying the relations of bodies to heat and electricity, was induced, by having in his possession a considerable number of large tourmalines, to repeat, and endeavour to verify, some experiments which Becquerel had made with this mineral. His method of experimenting was essentially with the electrometer of Coulomb; and his modification of the apparatus for the purpose of experimenting is, we think, as efficient as it is simple and elegant. To this we would refer such of our readers as are desirous of making experiments on the electricity of minerals; and merely confine ourselves here to a statement of his results.

He verified the conclusion of Becquerel, that the Tourmaline only develops electricity when it is in the act of changing its temperature. While the mineral remains stationary in temperature, not the slightest vestige of electricity is indicated by the electrometer; nor when placed in a cooler medium was the developement of electricity proportional to the change of temperature. It appeared to rise gradually to its maximum when the tourmaline was about half-way cooled to the temperature of the apartment; then gradually diminishing, re-descended to zero, when it reached that point. Such was Becquerel's experiment when the tourmaline was heated to 212°; and Professor Forbes has found the same general character to attend upon other temperatures.

He then made some experiments to ascertain the effect of *dimension* in modifying the electric action. In this he found great irregularities, which he attributes, with great probability, to irregularities in the structure of the specimens; but upon the whole he considers himself justified in believing that the "area of the section has so far a general influence, that where the differences are considerable, the thickest crystal has almost universally the greatest power." The maximum intensity of a long tourmaline, though less than with a shorter one, was considerable, but more slowly attained.

He also applied the same mode of experimenting to three other minerals—*topaz*, *boracite*, and *mesotype*. To these the law of Becquerel also extends—viz., that the maximum intensity of the developed electricity arrives when the rate of cooling has become comparatively slow. The topaz retains its electricity long after the temperature has ceased to change. He thinks it probable that the decomposition and recomposition of the electric elements is more difficult in larger than in smaller crystals; and that this may account for the anomalies observed by former experimenters to be presented by the tourmaline.

III. *A General View of the Phenomena displayed in the Neighbourhood of Edinburgh by the Igneous Rocks, in their Relations with Secondary Strata, with reference to a more particular Description of the Section which has been exposed to view on the South Side of the Castle Hill. By* Major-General Lord Greenock, C.B., F.R.S. Ed.

Lord Greenock considers that the trap-rocks in the neighbourhood of Edinburgh, formed by "lines drawn north and south from Burntisland to the Pentland Hills, and east and west from Salisbury Craigs to Corstorphine Hill, form anticlinal lines (or nearly so) from which these rocks will be found to decline in opposite directions." He then goes on to describe the geological appearances exhibited by cutting the new south-west road, in the section of the Castle Hill; and thinks they may all be explained by the hypothesis of two successive and very distant convulsions occasioned by the contraction of the interior crust of the earth during its earlier periods of refrigeration; the first of these, when the trap was formed in a state of igneous fluidity at the bottom of the sea—the second, when, with the whole district in which they are situated, they were lifted up to their present positions in a hard and consolidated state.

IV. *Description and Analysis of a Mineral from Faroe, not before examined. By* Arthur Connell, Esq., F.R.S. Ed.

Whether viewed in reference to its chemical or optical characters, this mineral differs from all others with which Sir David Brewster or

Mr. Connell were acquainted. It had been supposed to be a species of mesotype by Mr. Rose, the mineral-dealer; and Sir David Brewster appeared to think it might possibly be a variety of the apophyllite. Mr. Connell was led to believe, from his analysis of it, that it is essentially a hydrated quatersilicate of lime, and its constitution expressed by the formula $9\ S^4\ \dot{C}+16$ Aq., which gives—

Silica .... = 58·06
Lime .... = 26·85
Water .... = 15·08
99·99

The general analysis gave the following elements of its composition:

Silica .... = 57·69
Lime .... = 26·83
Water .... = 14·71
Soda .... = 0·44
Potash .... = 0·23
Oxide of iron .... = 0·32
Oxide of manganese = 0.22
100·44

It differs from the table-spar in containing water as an essential constituent, and in the relative proportions of silica and lime,—table-spar being a bisilicate of lime: from those zeolites which do not contain alumina as a necessary ingredient—such as the apophyllite and Dr. Thomson's Wollastonite—from which it differs in this respect, that the quantity of alkali is too small to be viewed as an essential constituent, and that the relative proportions of the silica and base are different.

Sir David Brewster made ineffectual attempts to obtain crystals from the surface of a large specimen which he possessed; for though the faces of the crystals are perfectly distinct, they lie so near the general surface that it was impossible to detach a single fragment without pounding it. He, however, had a piece of the mass ground and polished, and ascertained that it has a doubly-refracting structure. It is also opalescent, reflecting a blueish light, and consequently transmitting a yellowish one, and did not possess pyro-electricity. In a subsequent examination of a thin slice, it was found to polarize light *in all directions*, showing that the mineral consists of a congeries of crystals adhering together in all positions.

Mr. Connell proposes to call it *dysclasyte*, (from δυς and κλαω,) and of course to arrange it with the *zeolites*.

V. *Researches on the Vibrations of Pendulums in Fluid Media. By*
George Green, Esq.

The object of this paper is to apply the general equations of the motion of fluids to a determination of the circumstances which are to be considered in a pendulum vibrating in a fluid medium, and describing small arcs of vibration. He assumes an ellipsoidal bob; and finds, by a very elegant series of reasoning upon the formulæ of motion which this case presents, that it will not only be necessary to allow for the loss of weight caused by the fluid medium, but also to conceive the density of the body to be augmented by a quantity proportional to the density of the fluid. The value of the quantity last named, when the body of the pendulum is an oblate spheroïd vibrating in its equatorial plane, has been completely determined; and when the spheroïd becomes a sphere, is equal to half the density of the surrounding fluid. Hence, in this last case, we shall have the true time of the pendulum's vibration, if we suppose it to move *in vacuo*, and then simply conceive its mass augmented by half that of an equal volume of the fluid, whilst the moving force with which it is actuated is diminished by the whole weight of the same volume of fluid.

Mr. Green also shows that as the ellipsoïd is more oblate, the correction becomes less; and that in *elastic* fluids, the error to which his conclusions lead cannot have a much greater proportion to the correction which he assigns, than the pendulum's greatest velocity does to that of sound.

We cannot conclude this notice without expressing some degree of surprise at the notation for an arc employed by Mr. Green. We think it savours something of affectation in an analyst—and a *young* analyst, too—of his powers, to write arc ($=\tan a$) for $\tan^{-1} a$. Nor would it be just, to omit referring our readers to Professor Airy's important researches on the motions of a pendulum vibrating in a fluid medium, published in the *Memoirs of the Cambridge Philosophical Society* for 1826; though it would be out of place, on the present occasion, to enter into any special detail respecting them.

VI. *On the Force of the Latin Prefix*, væ or ve, *in the Composition of Nouns and Adjectives. By* the Rev. Archdeacon Williams, F.R.S. E., *Rector of the Edinburgh Academy.*

Amongst the monuments of the ancient states of human society, the disjointed and time-worn fragments of speech, the *débris* of a former state of mental existence, we often find a clue to the decision of questions which have made their appeal to other tests in vain. Before the rise of the accurate sciences, the history of languages is the history of the human mind, and, in no small degree, the history of human society too.

A language in its infancy has but few radical terms, and these are confined to the expression of our common feelings and actions, and to the names and qualities of the objects with which its inventors are conversant. It is a well-established fact, that the least complex modulations of sound are common to the whole human family: and that there is no one, however artificial, which (except in special cases of physiological imperfection of vocal structure.) cannot be acquired by the inhabitants of any country on the globe. It is true, there may be a *general habit* as to mode of enunciation, a sort of thorough-bass of speech, which may always render it a matter of difficulty to acquire the precise manner of intonation which may prevail amongst another people. For instance, a Frenchman may find it extremely difficult to enunciate the *θ;* but does not this arise entirely from the habit of his native language, which renders all his terminations harsh because of their rapidity? When, again, do we hear a Cambrian, a Scotchman, or an Irishman, lay aside the peculiar intonation of his country? Or, to come nearer home, do we believe that an omnibus cad, whatever may be his good luck in life, and the society into which money may introduce him, would cease to talk of the "*Elephant-an-cawsle*"? Yet these variations are scarcely less marked than the national peculiarities of general intonation which may be found all over Europe. We have heard of Frenchmen speaking English so as not to be distinguished from a native of the "Sea-girt;" and we have also heard of the same facility existing amongst Englishmen with respect to other European languages: but we have only heard *of* them—not heard the thing itself.

However this may be, we are quite disposed to believe, with Archdeacon Williams, that the essential elements of human speech, in the great Caucasian family, are indestructible: and we are so far from wishing to discourage such investigations, that we hail as one of the signs of the times very favourable to the preservation of the *true science of mind*, the investigations which so many profound and philosophical scholars, both in this country and throughout the Continent, are daily laying before us. It is with this feeling, that we approve of the researches contained in the Archdeacon's paper; and that we earnestly recommend the attention of our *scientific* readers to this class of inquiries.

The author remarks on this subject:—"This can be truly said of the particle *ve*, or rather *væ*, which still exists in the language of the Scottish peasant, under the same form, and, as far as can be ascertained, almost with the same sound, with which it was pronounced more than twenty-five centuries ago in the vicinity and streets of ancient Rome. But," says he, "I would not be understood that Scotland alone has retained the word, which, on the contrary, has been, or still is, a useful portion of many other languages, as may be seen from the following table, where the precedence is given to those dialects which have retained the original guttural sound, more or less softened:—

Cimbric - - -	Băch, Văck, softened Vā	Small, little.
Gaelic - - -	Beag	
Persian - - -	Bega, Beja	
Teutonic - - -	Fahe	
Greek - - - -	Βαι,—ος	
Scottish - - -	Wee	

To prove that the Latin *ve* bore the same meaning, and was, in fact, cognated with these, he proceeds to examine every word to which it is prefixed in its pure state as a long syllable. These words are, *Vecors*, *Vedius*, *Vejovis*, *Vepallida*, *Vesanus*, *Vescus* and *Vesculus*, *Ve-sbius*, *Vesica*, *Vespa*, *Vesperus* and *Vesperugo*, *Vespices*, *Vestibulum*, *Vestigium*, and *Vegrandis*.

His conclusions, though opposed in some cases to high authorities, are, we consider, fully made out by a complete induction from all the cases in which the prefix occurs, that *ve* never has an extensive power, Reichenbach published three successive papers, one in 1830, a second in 1831, and a third in 1833. In the first he describes the *paraffine*, as white, tasteless, inodorous, rather tough, lighter than water, fusible at 130° or 135° Fahrenheit; and it distils unchanged at a higher temperature.

It resists the action of the strongest acids and alkalies; and finally, when pure, it burns with a bright flame without smoke.

In the second memoir he described another product of the destructive distillation, which he called *Eupione*. This body is a liquid, colourless and tasteless, but of a fragrant odour. It is remarkably fluid, and is the lightest liquid known, under the ordinary pressure having only a specific gravity of ·655. It boils at 110°, distilling unchanged. Like *paraffine*, it resists the strongest acids and alkalies, and burns with a bright white flame without smoke*.

An idea had become general, that native naphtha was the product of destructive distillation, and of course Reichenbach would expect to find these products in it; but, to his surprise, he could not distinguish a single trace of either. He attributed his failure to the difficulty of procuring genuine naphtha, and conjectured that oil of turpentine had been employed to adulterate the specimens he examined, this possessing in a high degree the peculiar odour and other properties of the native naphtha. In his third *Mémoire*, he returned to the subject, and after examining the best naphtha he could procure, he was still unable to detect the minutest quantity, either of *paraffine* or *eupione*. This led him to question the general opinion, and to suspect that, after all, naphtha itself might not be a product of destructive distillation. To put this to the test, he distilled large quantities of brown coal along with water, at 212° (and consequently, no destructive distillation could occur), and obtained considerable quantities of a naphtha which agreed in all its characters with that which he had previously examined. It contained not a trace of paraffine or eupione, and, to his surprise, presented the peculiar odour of oil of turpentine.

Upon comparison of the three bodies, the naphtha of his own manufacture, the naphtha which he had before experimented on, and oil of turpentine, he found them all to exactly agree in specific gravity, their boiling-point, and their chemical properties. From this remarkable coincidence he drew the conclusion that the native naphtha which he had examined was genuine; and that it was nothing more than the oil of turpentine of the great pine-forests, to which our coal-beds owe their origin, having been separated by a gentle heat, either before the conversion of wood into coal, or from the coal itself, as in his own experiments.

In this stage Dr. Gregory entered upon the inquiry, being much struck with the results of Reichenbach, and yet convinced that the Rangoon and Persian petroleum must be a different substance from oil of turpentine and the artificial naphtha which he had formed; and his expectations were realized by finding a considerable quantity of eupione in the Rangoon as well as well as in the Persian naphtha. As Dr. Christisen had also obtained *paraffine* from it, there could be no longer any doubt as to these oils being the product of destructive distillation. He also found that a liquid, sold in Paris under the name of naphtha, for the preservation of potassium, also possesses the same properties; and all three were submitted to comparative tests with oil of turpentine, native naphtha, and the artificial naphtha of Reichenbach, and many discriminating features in respect to combination with other bodies are pointed out.

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* These characters were not fully and in all respects developed in the *Mémoire referred to; but are partly taken from* subsequent determinations given in Schweigger's Journal for April, 1834.

The existence of two kinds of naphtha, one the result of destructive distillation, the other not, is therefore fully made out.

Also, that the naphtha examined by Reichenbach, as well as oil of turpentine, differ essentially from those of India and Persia, which are decidedly not oil of turpentine.

The fact of oil of turpentine having been obtained from the distillation of brown coal at 212°, proves that the coal had not been previously exposed to a heat sufficient to expel its oil of turpentine, and, *à fortiori*, that it had never been subjected to destructive distillation.

We would suggest the importance of an examination of the *anthracite*, in connexion with an opinion entertained by many, that this coal has been carried up by the lava which forms the trap-rocks. Upon the decision of this question, an important geological hypothesis must stand or fall. The products, it is true, are gone, but the residuum may be compared with the residuum of destructive distillation.

Dr. Gregory concludes with some judicious remarks on the commercial value of the Persian and Rangoon petroleums, for the purposes of affording light. *Paraffine*, he remarks, equals wax in the whiteness and purity of its light, while it is much more permanent and indestructible; and there is no fluid used for lamps at all comparable to *eupione* in these properties. Moreover, both these substances are entirely free from any disagreeable smell when burnt.

XI. *On the Refraction and Polarization of Heat. By* James D. Forbes, Esq., F.R.SS., London and Edinburgh, *Professor of Natural Philosophy in the University of Edinburgh.*

The chief results contained in this paper have already been laid before our readers, in our first Volume.

XII. *On the Fresh-water Limestone of Burdiehouse, in the neighbourhood of Edinburgh, belonging to the Carboniferous Group of Rocks. With Supplementary Notes on other Fresh-water Limestones. By* Samuel Hibbert, M.D., F.R.S.E., &c. &c.

XIII. *Analysis of Coprolites and other Organic Remains imbedded in the Limestone of Burdiehouse, near Edinburgh. By* Arthur Connell, Esq., F.R.S.E.

It would be impossible to analyze these two papers in less than a sheet of our work. No geologist can, with justice to himself, pass them by without the most attentive study, whether he derives the same conclusions from them that the authors do, or not. The results of the observations made are in every way remarkable.

XIV. *On Water as a Constituent of Salts.* 1. *In the Case of Sulphates.* *By* Thomas Graham, Esq., F.R.S.E., *V. Pres. Phil. Soc. Glasgow.*

This paper is entirely theoretical, and intended to establish the position that, when after the usual process of expelling the water of crystallization at a comparatively low temperature, one atom of water still remains, which resists expulsion at a much higher temperature, then this atom performs the functions of an acid. He illustrates his theoretical views by several examples drawn from different chemical compounds; but as neither the investigations, nor, perhaps, the whole of his arguments, are yet before us, we shall defer any analysis of his views till we come to the succeeding part of the volume. The inquiry is an important one, and we shall avail ourselves of the appearance of the succeeding half-volume, to give as complete an account of it as our limited space will allow.

THE NOVEMBER-ASTEROÏDS.

The communications recently received by M. Arago from various parts of France, detailing the appearances of shooting stars on or about the night of the 12th-13th of November last, and the observations made at the Observatory of Paris at that period, under his own direction, have increased to a high probability, the notion that these meteors are not isolated vagabonds in space, glittering accidentally and at uncertain times, but that they are regularly periodical,—are grouped in companies, greater or less in number, and together constitute a distinct member of that system which includes the Earth and the other planets. Nearly two years ago M. Arago was so impressed with their remarkable character, that he, even at that very early period, designated them as Asteroïds*, thus classing them with Ceres, Pallas, &c.

In that part of the instructions to the *Bonite* which were drawn up by him, and in the scientific article attached by him to the *Annuaire* of 1836†, he inserted a notice of "the astonishing appearance of bolides *(fiery meteors)* observed in America on the 12th and 13th of November, 1833," and called for vigilant observations of shooting stars in general, and for a very attentive watching of the sky during the absence of the sun, from the 10th to the 15th of November in future years.

He followed his own precepts, and as early as the 14th of November he thus describes, at a sitting of the *Académie des Sciences*, the arrangements and observations made, on the days immediately preceding, under his care, at the Observatory of Paris.

"As the instructions delivered to the officers of the *Bonite* invited *navigators* to observe attentively all meteors which might occur in the sky, from the 10th to the 15th of November, and particularly during the night of the 12th-13th (the usual time of a very extraordinary shower of shooting stars), it was confidently anticipated that all *resident observers* would consider the request as also made to them.

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* See page 392 of the last volume.

† Given complete in this Magazine.

" I therefore did not neglect to desire the four young astronomers whom the *Bureau des Longitudes* has placed under my care, MM. Mauvais, Bouvard, Laugier, and Plantamour, to keep watch by turns on the upper terrace of the Observatory, from sunset to sunrise each night from the 10th to the 15th of November, for the purpose of accurately noting the instant of the appearance of any bolide, the direction of its course referred to fixed stars, its duration, and its brilliancy.

" Unfavourable weather interfered with these observations until the 12th; but on the evening of that day the sky cleared, and between $6^{h.}\ 48^{min.}$, P.M. mean time, of the 12th, and $6^{h.}\ 35^{min.}$, A.M. of the 13th, 170 of these meteors were observed. This would give, on an average, rather more than one bolide for every four minutes; but it should be remembered, that some of the feeblest of the meteors might be lost in the twilight of the morning.

" In the middle of the night, from $11^{h.}\ 45^{min.}$ to $2^{h.}\ 5^{min.}$, viz. in $2^{h.}\ 20^{min.}$, 71 shooting-stars were observed, being, on an average, one in every two minutes.

" Of the 170 meteors above mentioned,—

" 52 had their track in the constellation Leo;

" 73 moved in directions, which, if prolonged, would have traversed the same constellation;

" 40 were in courses that had no tendency towards that constellation; and

" 5 appeared but for an instant.

" On the preceding evening, between ¾ past 11 and ½ past 12, not a single meteor was observed!"

At the sitting of the 28th of November, M. Arago presented the following abstract of the information received from several quarters relating to the asteroïds on the 12th-13th.

PARIS AND THE NEIGHBOURHOOD.—At the Observatory, as has been said, there were seen in about 11¾ hours 170 shooting-stars, whose directions have also been described.

At *Bercy*.—M. Meret, though thwarted by mists, which, in this quarter, occupied the lower region of the sky, yet saw between midnight and 6 A.M. 120 shooting stars. The directions of 84 only were remarked. Of this number, 57 proceeded either from the constellation Leo, or ran in lines which, if prolonged, would have crossed it.

M. Charles Coquerel was on the watch from 4 to 6 o'clock only, but in this short interval he saw at least 26 shooting stars. These meteors in general were in lines which, if prolonged, would have passed through Leo. A few, however, in the east, shot along horizontal lines, at an altitude of about 45°. All, without exception, moved towards the north.

LA CHAPELLE, near Dieppe.—MM. *Racine* and *Calais* observed the phenomenon under the inspection of M. *Nell de Breauté*. Between $11^{h.}\ 39^{min.}$, P.M. and $3^{h.}\ 24^{min.}$, A.M., *i. e.* in $3^{h.}\ 45^{min.}$, they saw 36 shooting stars.

Yon-Altemare, Department de l'Ain.—M. Millet d'Aubanton, between 8, P.M. and 6, A.M., *i. e.* in 10 hours, saw 75 shooting stars.

Strasburg.—Prof. Fargeau, assisted by three of his pupils, saw, between 10$^{h.}$ 45$^{min.}$, P.M., and 2$^{h.}$ 37$^{min.}$, A.M., *i. e.* in 3$^{h.}$ 52$^{min.}$, 85 shooting stars. The Professor believed that he did not note near the whole number of meteors which appeared. The courses of 57 out of the 85 pointed to Leo. Of the remaining 85, many probably took the same direction, though they were not minuted.

Mme. Glück and M. Hohl saw, between 10$^{h.}$ 20$^{min.}$, P.M. and 2$^{h.}$ 25$^{min.}$, *i. e.* in 4$^{h.}$ 5$^{min.}$, 28 shooting stars.

Arras.—Prof. Larzillière, who observed but occasionally, and sometimes from his chamber-window only, so that he evidently commanded but a partial view of the heavens, yet between 3 o'clock and 6 saw 23 shooting stars. 18 of these moved in lines that either had their commencement in Leo, or which, if prolonged, would have traversed it.

Angers.—Prof. Morren did not observe during the whole night; but between 2$^{h.}$ 20$^{min.}$ and 4$^{h.}$ 21$^{min.}$ he saw 49 shooting stars. "All my attention," says M. Morren, "being directed towards the constellation Leo, a great part of the heavens was necessarily disregarded by me."

Rochefort.—Between 1$^{h.}$ 30$^{min.}$ and 3$^{h.}$ 30$^{min.}$, Lieutenant Salneuve observed 23 shooting stars.

Havre.—M. Colback, merchant, estimated that between 9 P.M. and 2 A.M. he saw, on an average, one meteor per minute; at the latter hour the sky became covered with a slight mist.

Among other places where the meteoric appearances were unusually brilliant on the same night, but where the bolides were not counted, nor observed with precision, may be mentioned, *Nogent-sur-Vernisson*, the neighbourhood of *Tours*, the valley of the *Rhone*, &c.*

Are the Asteroïds which have been observed on the 12th-13th November to be considered as remarkable and unusual phenomena?—Let us compare the number of asteroïds seen on the nights preceding and following that of the 12th-13th November, 1836. At the Paris Observatory, during the night of the 12th-13th, were seen, on an average,

14	asteroïds in	1 hour
29	”	2 hours
43	”	3 ”
58	”	4 ”

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* At Highgate, near London, a small party of observers had prepared to observe during the night of the 12th-13th of November. The heavens, at first, were for the most part clear, and the weather favourable. Two shooting stars were noted between 9-10 P.M. but clouds then came on from the north, and about 11 the sky was entirely covered. Soon after a south-west wind arose, which gradually increased to a gale, and, accompanied by rain, continued till after day-break.

From the testimony of M. Ed. de Verneuil, who lately returned from a geological tour in Turkey and Southern Russia, *it would seem that* the appearance of the phenomenon was not universal. In a note to a Parisian editor he says, "It may not be uninteresting to you to know, that this phenomenon did not take place in Hungary. I passed the night of the 12th-13th of November on the road from Pest to Vienna, in a manner the most favourable for observing everything which passed; for I travelled in the open wagons of the country, which were changed at every stage. I was accompanied by Mr. Harcourt, an English barrister, and M. Negropont, a resident of Athens, and not one of us saw, during the whole night, the smallest luminous meteor."

In the night of the 11th-12th, not one was observed!—
,, 13th-14th, 3·5 only per hour.
,, 15th-16th, 2·3 ,,

The conclusion is evident.

The observations of M. Méret are not less conclusive; they give, as a mean,

20 asteroïds in 1 hour
40 ,, 2 hours.

But on the 11th, from half-past 7 to half-past 9, that is to say, during *two hours*, M. Méret did not see one meteor.

Let us take the observations of M. Millet.

M. Millet saw, on an average, during the night of the 12th-13th,

7·5 asteroïds in 1 hour
15 ,, 2 hours
22 ,, 3 ,,
30 ,, 4 ,,

But, on the 6th of the same month he saw *none* during 2 hours
7th ,, ,, 4 ,, 4 ,,
8th ,, ,, *none* ,, 3 ,,
9th ,, ,, 1 ,, 6 ,,
14th ,, ,, 2 ,, 6 ,,

We shall not mention here the direction of the courses generally taken by these asteroïds as a characteristic distinction of the phenomenon of the 12th-13th November, principally because, apprized by the *Bonite-Instructions*, special attention would be directed to the quarter of the heavens in which the constellation Leo would be found; and it would therefore follow, that observations of this quarter would be more numerous and complete than any other, even if the meteors had been uniformly distributed over the whole of the heavens. But a consideration not less important is, that in each year, the directions which these asteroïds have usually taken, appear to be included in the semicircle diametrically opposed to the motion of the earth in its orbit*. Now, on the 13th of November, the earth is in that tangent to its orbit which points towards the constellation Leo; it would therefore be *from* this constellation that the greatest number of these asteroïds would *appear* to move on the 13th November, even upon the hypothesis of their motions being really disseminated in every direction. A very exact observation of all these motions in future, would throw a light upon the phenomena of these asteroïds, which is altogether wanting at present.

Observations, more numerous and complete, combined with calculations for parallax, will determine if all the directions of motion exist in the same degree in the zone of asteroïds traversed by the earth on the 13th of November, or if a simple conical flux would be sufficient for the explanation of the phenomenon, &c. Before these facts are obtained, it would be premature to attempt to ascend to the physical cause of these curious appearances: to inquire, for example, if, in order to account for them, it would be necessary to suppose that a large planet had formerly exploded into millions of fragments, at the very moment when it occupied the place which the earth now arrives at on the 13th of November—that these fragments follow the same laws as the molecules which compose the tails of comets, &c.

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* See p. 391 of the last Volume.

Physical Nature of the Asteroïds of the 12th-13th of November.—The asteroïds of the 12th-13th of November are evidently only visible when they penetrate the atmosphere of the earth. The most brilliant of those in 1836 shed a lustre which might be compared with that of Venus. All, or nearly all, left a train of sparks after them, visible 1—6 seconds. In some cases, a space of not less than 25° was passed over between the place of their appearance and that of their total extinction.

If it were certain that some of their motions were sinuous, we should be inevitably led to the conclusion, that the matter of the asteroïds of the 12th-13th November must have had a very small density.

The last appearance of these asteroïds has proved beyond contradiction, that they sometimes fall upon the earth. M. Millet, in fact, says, he saw several of these meteors strike the slope of the mountains with which he was surrounded. Captain Berard saw one of them, at Paris, descend as low as the parapet of the Pont-Royal.

Professor Olmsted[*] is of opinion, that these meteors have their origin beyond the limits of our atmosphere,—that they consist of light, transparent, combustible bodies, which take fire on entering into the atmosphere, having existed together in space, in the form of a cloud, or nebulous body, of great extent, and that this cloud, or nebulous body, has a periodical revolution around the sun, constituting a distinct member of the solar system.

M. Biot, who has thought the subject worthy of considerable attention, read an elaborate paper upon it before the *Académie des Sciences* on the 5th of December last. In this he argues, that the phenomenon might be produced by the rencontre of the earth, in its course, with the extremity of the solar nebulosity to which the zodiacal light has been attributed. After stating the facts on which this opinion is founded, and supporting it by a masterly train of reasoning, M. Biot observes,—" I do not conclude, from the preceding considerations, that the meteor of the 13th November is absolutely produced by the rencontres and perturbations of certain parts of the solar nebulosity by the earth. I have been only desirous to show, that on the 13th November the earth arrives very near to the ascending node of the nebulosity, towards which she is approaching, and across which she will soon travel; that in these circumstances of position and of motion, she must certainly operate, by her attraction and by her collision, on the material particles of the nebulosity, which will be at the same epoch near the ascending nodes of their orbits, and at similar distances from the sun as the earth, or very little different, from which would result phenomena that would be coincident as to direction and time, with those that the periodical meteor of the 13th November has presented. Finally, I have pointed out that the usual and much more central passage of Mercury and Venus through the region of the nebulosity, must have dispersed (and probably will again disperse) innumerable multitudes of its particles into orbits slightly inclined to the ecliptic, and in every direction, so that the earth may accidentally meet with them again in other points of its course. Although these deductions appear obvious to me, and that they must necessarily follow from the facts and the laws of attraction, I yet offer them with great hesitation, knowing how easy it is to be deceived in similar questions by the most probable analogies, when we are not able to prove their reality completely by exact calculation. But it should be remembered we can scarcely ever make a step in advance in the physical sciences, nor dare to anticipate remote relations, if we never attempt to connect facts except when calculation can be rigorously employed for that purpose."

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[*] Professor Olmsted was the first to call the attention of *physiciens* to the phenomenon. In articles published in *Silliman's Journal,* vol xxv., p. 363, vol. xxvi., p. 132, and vol. xxix., p. 376, in the *years 1834 and 1836,* he has given very minute accounts of the times and places of their appearance, and treated largely on their probable cause, nature, &c. We have in the present article, and in the *Bonite-Instructions* given all that M. Arago has published.

Notice of Bolides, &c. which have been observed about the 12th-13th of November, in Years previous to 1836.

The following notes have been collected from authorities that may be depended upon. They can hardly fail to be interesting at this period of the inquiry.

1634. *13th November.*—A large ball of fire, with a long tail, was seen between Joachimsthal and Gottes-gabe, in Bohemia.

1761. 12th. A large fire-ball exploded with a prodigious noise, and set fire to a house near Dijon.

1791. 12th.—Thirty-nine minutes past six, A.M., a luminous meteor resembling a squib, or rocket, was seen by Lichtenberg, at Göttingen.

1799. 12th.—Six A.M., a globe of fire appeared with a strong hissing noise, and finally exploded. At this epoch, also, meteors were seen by Humboldt and Bonpland[*] in America, by the Moravian Fraternity in Greenland, and by various persons in Germany.

1803. 13th.—Half-past eight P.M., a very remarkable fire-ball was seen in England.

1813. 10th.—Forty minutes past six, P.M., a luminous and detonating meteor was observed in England. On the 8th, an unusual blue light, which exceeded that of the moon, was remarked; this was probably an aurora borealis. In the evening of the same day many shooting stars were seen.

1818. 13th.—Quarter past seven, P.M., a meteor diffusing a blue light, and accompanied with sparks, appeared in England.

1819. 13th.—Seven P.M., a very extraordinary globe of fire appeared at Haïti; another on the 14th was seen in Bohemia; and a third, on the 18th, in England. Lastly, on the 19th, a meteor, accompanied with a large number of shooting stars, was observed by Bowditch in North America.

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[*] M. Biot is disposed to believe that these observations of Humboldt and Bonpland, on the 12th November. 1799. were the first that were made of this phenomenon. They are described in the historical part of the *Voyage aux Régions Equinoxiales*, vol. i. p. 519.

1820. 12th.—Four P.M., a globe of fire, accompanied with thunder, was seen in the province of Kursk, in Russia.

1822. 11th.—A globe of fire was seen at Freyberg; on the 12th, a large shooting star at Potsdam; and, on the 15th, a fire-ball at Apenrade.

1824. In the night of the 13th-14th, a globe of fire was observed at Mentz.

1825. 14th.—Eight P.M., an igneous meteor, which exploded, was observed in Scotland.

1831. 12th-13th.—Captain Berard, at sea, within sight of the Spanish coast, near Carthagena, saw, for the space of at least three hours, two shooting stars per minute; many of these were of an extraordinary size, and left behind them trains of iridescent light. Dr. Wright, of Ohio, United States, observed a shower of shooting stars on the same day of the month in that state.

1832. 13th.—Between three and four, A.M., at Orenburg, in Asiatic Russia, a large number of shooting stars were seen taking a direction from north-east to south-west; between five and six o'clock the phenomenon was at its greatest splendour; it continued till sun-rise. In the same night, and about the same hours, a luminous phenomenon was observed at Hitzkaza-Saschtschita, about 360 miles to the south of Orenburg. This would appear to have been an aurora borealis.

1834. In the night of the 12th-13th, a large number of shooting stars were seen in several of the United States of America; and their appearance produced a discussion between several *physiciens* of that part of the world. It was at this time that Professor Olmsted, of Newhaven, U. S., demonstrated the analogy of this phenomenon with that of the preceding year.

1835. In the night of the 13th-14th, M. Millet-D'Aubanton, in the department of L'Ain, saw an aërolite, which, on its fall, set fire to two houses; M. Delezenne, at Lille, observed a shooting star, larger and more brilliant than Jupiter, and which left on its track a train of sparks. A great number of the same kind of meteors were seen in the United States, and, in the other hemisphere, by Sir John Herschel at the Cape of Good Hope.

METEOROLOGICAL RETROSPECT

OF THE YEAR 1836.

THE year opened with a very sharp frost; on the morning of the 2nd of January the thermometer stood at 12½° (= 19½° below the freezing-point); this was however but of short duration, for on the 5th, only three days after, it marked 50° in the shade, and with the exception of a moderate fall of snow on the 11th, and a few frosty nights, the month of January was mild. It was also dry. The greatest *pressure* for the whole year took place on the morning of the 2nd of this month.

The 17th and 18th of FEBRUARY were noted for being remarkably tempestuous, with the wind at north and north-west; the latter part of the month proved cold, with night-frosts. The *minimum* for the year was brought on, on the 2nd of this month, by a southerly wind.

More than an average quantity of rain fell in MARCH, and there was a great range of the barometer; this month had but two or three frosty nights. The wind invariably from west and south.

APRIL was much colder in proportion than MARCH, particularly at the close of the month, when we were visited with frost and snow, and strong northerly winds, and the thermometer on the 30th at *sunrise* stood below 28°, doing a great deal of injury to the tender vegetation; all fruit-trees suffered from the inclemency of the period; it was succeeded by a fine clear MAY, with an uninterrupted series of easterly winds, and a dry JUNE, but not hot. Vegetation scanty and unhealthy, and grass dried up.

The first ten days in JULY unusually hot,—the thermometer at 87° in the shade. Strong gales from the Atlantic cooled the air considerably, and on the 20th of the same month the thermometer had sunk to 49° at one, P.M. Some thunder-storms in the neighbourhood of London, but none of any importance passed over Greenwich.

AUGUST was on the whole the finest month in the year; SEPTEMBER was cold, a series of northerly winds prevailed about the middle of the month, and a cloudy sky reduced the heat *below* the mean.

OCTOBER was remarkably wet and tempestuous, above 5 inches of rain fell; the 1st, 6th, 11th, 12th, were noted as being particularly stormy. Some fine auroras appeared, those on the 11th, 15th, and 18th in particular—but the most remarkable feature which this month presented was the fall of snow on the 29th, and which lay on the ground to the depth of six and eight inches for two or three days. A few frosty nights succeeded this deposition in the early part of NOVEMBER; these were followed by a succession of S. W. winds and small rain till the great tempest on the 29th. It appears from all accounts that the violence of the wind on that day exceeded anything of the kind known for many years; the storm passed over the whole of the South of England, Ireland, and the northern part of France. Many thousand trees were destroyed. Houses were unroofed, stacks of chimneys, &c., thrown down. Stacks, ricks, fences, &c., were scattered, and many solid edifices seemed to feel the effects of this singular *hurricane*. The movement of the barometer during the storm was a circumstance well worth recording;—on the morning of the 29th it rapidly sank till about noon, when at its lowest (28·85 inches) it suddenly rose; by half-past two o'clock, P.M. it had risen above half an inch; the exact rise in that short interval of time, viz. *two hours* and a half, was 0·54 of an inch; the surface of the mercury was much agitated at the time the wind blew with the greatest power, some of the oscillations equalled a *tenth of an inch*, the mercury in the tube appeared to *boil*. It was a few minutes after the greatest depression had taken place that the storm was most violent—by four o'clock in the afternoon a calm had succeeded.

As might be expected, DECEMBER set in with a high temperature; the thermometer ranging above 50° day and night, the S. W. winds pertinaciously prevailing; without an hour's respite from them till the shortest day; when on the night of the 22nd, during a heavy shower of hail, the wind suddenly got round to the north, accompanied by a falling barometer and a *decreasing* temperature. This introduced the memorable snow-storm, by which all communication with the metropolis by land was suspended for nearly a week; the drifts of snow were said to be in many places fifteen and twenty feet deep, perhaps taking a fair estimate throughout the country, there might be on an average from three to four feet. The gale from the north-east was equally uncommon for its violence, and caused great destruction among the shipping on the eastern coasts; nevertheless the frost was very moderate, to the end of the year; the thermometer being about 30°. It may be well to add, in conclusion, that on the 2nd of January the frost broke up, the S. W. winds again resumed their former prevalence, and we have as nearly as possible the same season as before.

I should wish to turn the attention of meteorologists to the uncommon duration and *force* of the south-west winds for the last three or four years; above three-fourths of the winds have set from that quarter,—while periods of twenty and even thirty days have passed without a change of wind, this will, of course, account for the very high winter-temperature we have latterly experienced,—it being nothing uncommon even in January for the thermometer to stand for several hours at 50°, and it is only by the sudden intervention of a northerly current now and then, that a fall of snow or a frosty night can be expected to succeed.

MISCELLANEA.

Mode of ascertaining the Heights of accessible Clouds.

Gentlemen residing in mountainous districts, where the clouds sometimes form on the sides of the mountains, should ascertain the perpendicular height of these clouds at their base, and see whether they are one hundred yards high for every degree of Fahr. which the temperature of the air is above the dew-point at the moment of their formation.

If gentlemen have no means of taking the dew-point directly, the following method will be found equally correct in ascertaining the height of the base of these particular clouds, at any time of the day, for the height varies every hour. Swing a thermometer (Fahr.) rapidly in the air to avoid the effect of radiation, note its temperature, then cover its bulb with a wet rag, and swing it as before until it sinks as low as evaporation can make it; then divide one hundred and three times the difference of these temperatures by the wet bulb temperature, the quotient will be the height of the base of the clouds in question, in hundred yards. For example, suppose the dry bulb is 56½° and the wet one 51½°, then the base of the clouds will be 1000 yards high. This height is calculated on the supposition that air cools at 4° Fahr. in ascending to a height where the baromer would be one inch lower than at the surface of the earth, and 4° more for every additional inch. If this latter law is not strictly correct, the height of the base of the cloud in question will vary accordingly, and the law itself may be accurately investigated by this method; for the precise degree of refrigeration necessary to condense vapour at a particular dew-point is known, after making an allowance for the expansion of the vapour itself and the fall of the dew-point on this account. As the discovery of a method to ascertain by the thermometer the height of a particular kind of cloud easily distinguishable from all others, is a matter highly curious in itself, independent of its connexion with the theory here advocated, it will no doubt receive that immediate attention which it deserves.—**Espy.** *Frank. Journ.*

Circuitous Course of Violent Winds.

Speculative opinions upon the course of a storm are usually, if not always, founded upon the erroneous notion of a rectilinear course in the wind. In the accounts received of a hurricane at Barbadoes on the 3rd of September, 1835, which raged for a few hours from E.N.E., fears were expressed for the safety of the islands to the northward; but subsequent intelligence from Guadaloupe and Martinico showed that the gale had not extended to these islands. Had the direction and phases of the wind been viewed in their true relations, it would have been perceived that the heart of the gale must have passed to to the southward of Barbadoes; and as a general rule in the West India latitudes, when the onset of the storm is found to be in the general direction of the trade-wind, or more eastward, the observer may consider himself as under the northern verge of the gale; but if the onset of the gale be from the northwestward, veering afterwards by the west to the southern quarter, the heart of the storm will be found to have passed to the northward of the point of observation, the latter being under the southern margin of the gale.

Among other proofs of the circuitous action of violent winds is the fact, that the track of a vessel which runs directly before the gale, will in many cases be found to be strikingly curvilinear when traced on the chart; in other words, the veering of the wind which so often occurs, when duly considered, is in itself a complete demonstration of the fact in question. Many readers will recollect the case of a vessel driven from Falmouth in the great hurricane of 1703, by a circuitous course to the Isle of Wight, with only a cabin boy on board, which course clearly indicates the phases of one marginal section of that memorable storm. It can but seldom happen, however, that the track of a vessel which scuds through a gale, will fully develop the entire circuit of the wind, the combination of circumstances necessary to this result being but rarely encountered. Still I have obtained notice of a few such cases, and a respectable ship-master not long since informed me that he once scudded for twenty-four hours under a typhoon in the China Sea, and on its departure, found himself nearly in the position where he first took the gale.—REDFIELD, *Sillim. Journ.*

Sèvres Museum for the Illustration of the Art of Pottery.

M. BRONGNIART is preparing a History of the Art of Pottery, and making great exertions for the formation, at Sèvres, of an extensive and instructive collection of every thing relating to the art. He has distributed a circular throughout America, which Professor Silliman has translated, and inserted in his Journal, in order to promote M. Brongniart's object. We insert it as a useful model of such letters missive,—as evidence of the comprehensive views of the collector,—and of the relaxation of the French government with regard to the customs' regulations in scientific importations. Both art and science have hourly to regret the obstructions and losses produced by the interference of the Custom-House officer.

In a letter to the editor of Silliman's Journal, dated March 8th, 1836, M. Brongniart remarks:—"I am much occupied with a work upon the history of the plastic art, or the art of pottery; and the requests which I take the liberty to annex, have for their object the enriching of a grand and instructive collection which I have formed at Sèvres, of every thing relative to the art of pottery."

Royal Manufactory of Porcelain, and for Painting on Glass.—Sèvres, March 8th, 1836.

UNITED STATES OF AMERICA.

(Circular.)

Instructions as to the manner of cooperating towards the completion of the collection relative to the arts connected with the manufacture of porcelain, and with vitrification, founded at the Royal Manufactory at Sèvres, near Paris.*

I.—What kinds of pottery are used by the different classes of inhabitants of the country; the agriculturists, the mechanics, citizens, and merchants, poor and rich?

Is the pottery of native or foreign manufacture?

If foreign, from what country does it come, and in what way?

If of native manufacture, where is it made?

II.—As to the native pottery, (and under this name we include all varieties, from the most common to porcelain,) it is desired to collect and procure specimens of every sort. Common pottery, both with and without glazing. Delftware common, and Delftware fine. Pottery of brown free stone; crucibles. Varieties of procelain. Bricks, both common and those manufactured by particular processes.

Plate species.—Plates, oval dishes.

Hollow ware.—Cups, salad dishes, tea and coffee cups.

Round pots, hollow-moulded.—Oval and square pieces, saucers, boxes, &c.

The largest piece of each sort that is made.

The name given in the country to each piece.

The price of each piece upon the spot.

Whether there is exportation, and to what place.

III.—FABRICATION.

1. *Materials for the mass or paste.* Clays. Marls, or plastic earths which may be substituted for them. Sands. Rocks or stones. Limestone.

For the glaze or enamel.—If stony materials—feldspar-stones.

If metallic matters—Metals, their oxides, and metallic glass.

Exact localities from which these materials are drawn.

2. *Modelling.*—Moulds of plaster, of terra cotta, or other materials of whatever kind.

The lathe and other instruments for fabrication.

Sketches, with exact dimensions of these instruments, if it is supposed that they differ from those used in Europe.

3. *Baking.*—Form of the ovens sketched, with the dimensions.

Combustibles used, indicating them in the clearest manner possible.

IV.—Information peculiar to the country.

1. To designate the principal manu-

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* The requests for information and for specimens apply equally to glass manufactures *and their productions.*

factures of pottery, glass, and porcelain, in your vicinity.

2. Whether there is in North America, ancient pottery; that is to say, pottery fabricated in remote ages, and which has not been made for a long time. This pottery is found, in general, in alluvial soil, in the ruins of towns, and perhaps, as in some parts of Italy —of South America, and—of the oriental countries of the ancient world, in the graves or tumuli. In Europe, these things have often been admitted into museums as monuments of antiquity, but almost never as in relation to the art of pottery and its history. It is in this latter point of view that I regard them, and that I have collected a great number of the ancient pieces of pottery in the museum at Sèvres.

To endeavour to collect some pieces of this antique pottery, and to indicate exactly the place and the circumstances in which they have been found, and to endeavour to decide whether it had anciently any celebrity, always however mistrusting the deception of the sellers.

3. Whether there is knowledge from traditions, inscriptions, &c., that the natives (aborigines) of North America have ever fabricated or known glass.

General instructions in relation to the purchase, packing, and forwarding of the objects collected.

The expenses which may be incurred in procuring the specimens and the information, will be reimbursed by the Administration of the Royal Manufactory at Sèvres upon a reference to the person who shall be designated to receive the amount.

It is expected that these expenses will not rise to a great amount: it is requested, in any event, that they may not exceed, in any one year, the sum granted, i.e. 200 francs for 1836, (£8): 200 for 1837; at least without a previous understanding with the Administrator of the Royal Manufactory at Sèvres.

It will be necessary to pack the pieces with great care, and to consign them to a merchant in one of the ports of France, to be forwarded by way of slow transportation to the Administrator of the Royal Manufacture at Sèvres; forwarding also the expenses of transportation.

It will be necessary that the correspondent at the sea-port should write a letter of advice *to the Administrator* of the Royal Manufactory at Sèvres, near Paris, before the forwarding, in order that the latter may obtain from the Director-general of the Customs permission for the box to pass under seal, (*sous plomb*,) and not be opened until it arrive at Paris: this is very important, to the end that there may be no derangement of labels, nor any breakage. It is equally important that the tickets which may indicate the places where the pieces were made, or those from which they come, should not be separated and mixed during the unpacking. It is desired, therefore, that they may be fastened either with glue, or with good wafers, or with twine*.

Lastly, it is very desirable that there should be attached† to the case a separate box, either of lead or of tin, or [and?] that there should be sent separately notes previously made of the objects collected and forwarded; taking care that a correspondence be established between the objects and notes, by means of a series of numbers, &c.

ALEXANDER BRONGNIART.

Shortest River in the World.

THE glaciers of Klofa-Yokul in Iceland approach the sea-shore, and at their termination have a breadth of 14 to 16 miles. A broad and rapid river, the Yokulsa, is generated in the dark womb of these immense deposits of ice, and rushes out from beneath them. In its course to the sea, which is *scarcely three miles*, it has no tributaries. Its temperature is nearly that of the freezing point.

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* Glue and wafers are both liable to be affected by humidity, and the first adheres badly to glass, and glazed surfaces. Wax-cements, which atmospheric changes do not make brittle or soft, are good. An excellent practice it to sow up the specimen and its label in a bit of cotton cloth, either with or without paper.

† There must be a mistranslation in this paragraph. The intention of M. Brongniart obviously is, that the box containing the specimens, should be inserted in, and not "attached to" another of lead or tin; and, no doubt, soldered up. Such a mode of packing is the best possible. At first sight, it would appear, that the inner box is unnecessary; but it is useful to prevent the crushing of the metal one. We have seen an instance, in which, for want of the inner one, a tin case was so distorted by pressure, as to destroy some valuable mineral specimens which it contained.

Congelation of Mercury by Natural Cold.

Extract from a minute of observations on freezing Mercury in the open air, made at Gardiner, Maine, United States, January 28th and 29th, 1817, by Mr. Ed. Hall, jun.

The whole of the day of the 28th was intensely cold. At 2 P.M., the thermometer hanging on the wall of a house stood at −6°. About sunset the wind subsided.

A tray of charcoal was placed upon the end of a wharf projecting into the Kennebeck, nearly a hundred yards from any building or other elevated object. On this was placed a thermometer in a blackened tin case, and two phials, each containing a small quantity of mercury, the lower half of each phial being blackened, and the phial a little raised from a horizontal position, so that the fluid might be within the blackened part. A similar phial of mercury was placed on the snow at a little distance; but as it underwent no change, no further notice was taken of it.

At 10 o'clock in the evening, the thermometer stood at −29°. The sky was perfectly serene and clear. At half-past 11, the thermometer had fallen to −32°. At half-past 3, (the 29th,) the thermometer was at −38°; the mercury in the phials of course still fluid. The atmosphere was remarkably transparent and perfectly calm. At half-past 6, the thermometer stood at −40°. It soon rose one degree while we were bending over to examine it; the mercury in the phials still fluid. I now poured out a small quantity of the mercury into an excavation in a piece of charcoal. At a quarter before 7, the thermometer was again at −40°; the mercury in the phials still fluid; but that on the charcoal was partially congealed. As I examined it with a slender stick, it exhibited the appearance of a soft solid, separating into parts without running into globules; and the fragments were rough, and evidently crystalline. These appearances, however, continued only a short time; but while I was examining it, being of course necessarily bent over it, the whole soon returned to a perfectly fluid state. At 7 o'clock, the thermometer was still as before at −40°. The mercury in the phials was unchanged. That on the piece of charcoal exhibited the same appearances as at the late observation, only in a less marked degree, and it sooner became fluid. Soon after this, the sun rose, and of course the attempt was discontinued.

A few weeks later, having been supplied by the kindness of the late Mr. Vaughan with several excellent thermometers of Troughton's manufacture, I attempted to ascertain how much effect was attributable to the cooling of the surface by radiation, in a similar state of atmosphere: and on one occasion found a difference of 18° between a thermometer on charcoal on the ground, and another suspended freely in the air, 18 or 20 feet above it; one being 36°, the other 18° below zero. This was a clear night, and subsequently, when clouds appeared, the difference diminished, until at the commencement of snow the two instruments agreed.—*Sillim. Journ.*

Efforts making to increase National Museums.

In speaking of the Paris Museum, I may refer to an advantage that collection has possessed which has not been enjoyed by any other; that of having all the specimens from the local [provincial?] collections in France sent to it, from which its professors selected what specimens were wanted for their collection, and the duplicates were then divided into series, illustrative of the arrangement of the animal kingdom, and one of this series was sent back to each of the local collections. This is an arrangement that could only be made in a country like France, where all such collections are public property. Napoleon, also, sent to it the specimens which were taken from the museums of the various towns overrun by his armies, few of which were returned at the peace; for even Holland was satisfied, after a time, to receive duplicates from the French collection in return for the specimens taken from the celebrated Museum at Amsterdam. Of late years the Dutch government have constantly employed eight travelling naturalists in various parts of the world, to replenish its collection; and the duplicates are used to increase their collections by exchanges with other museums, or are distributed to the local collections.

The Museums of Leyden, Berlin, and Vienna, also employ travelling collectors, but not to the extent of the French government. It is curious to observe the effect of this manner of obtaining specimens in the various institutions. The collection is generally richest in the productions of those countries where these collectors have been stationed: thus, the Berlin Museum is rich in Mexican, Cape, and Red Sea animals; the Vienna in Brazilian; and Leyden in Javanese, Japanese, and Cape specimens. The Paris Museum, from the number of its collectors, approaches nearer to the English collections, which entirely depend on the industry of travellers, or the enterprise of her merchants; and it is, I believe, from the great extent of her trade, that more specimens are brought to this country than to all the rest of Europe together, as the foreign collectors appear to have discovered, by the repeated visits they pay to England for the purpose of purchasing specimens.——GRAY *Analyst.*

Comparison of National Collections of Natural History.

WITH the advantages that the Paris Museum has enjoyed, it ought to be the richest collection in the world; but this is by no means the case: the display of *mammalia* at Leyden and Frankfort being certainly better. The collection of birds at Paris is very fine, but not very much larger than that of the British Museum, or of the Museum of the Zoological Society of London; and, it is said, considerably smaller than that of Leyden; but hitherto the birds in the Leyden Museum have not been exhibited to the public. The collection of shells at the *Jardin des Plantes* is not to be compared with some private ones in London, and is not so large as that in the British Museum.

It is not unusual to hear persons speak in raptures of the beasts in the gardens of the Paris Museum; yet it would be madness to compare them, in external appearance, to the collection of the Zoological Society, in London. I should not make these comparisons (for I consider these institutions as different in their constitution as is the character of the two nations) had I not constantly heard persons making similar comparisons, and almost always to the disparagement of the English institutions. In my repeated visits to the Continent, I have been induced to collect statistical accounts of the several institutions, and to observe their various peculiarities, with the view of introducing into the British Museum any real improvements I might discover.——GRAY, *Analyst.*

Bird Tracks in the new red Sandstone.

I HAVE discovered the tracks of birds in the new red sandstone; at Middletown, Conn., U. S., I have some well-marked slabs, with tracks. I find them in a sandstone lying *beneath* the new red fissile sandstone with the vegetable impressions mentioned in a former letter. This sandstone with bird-tracks is *perfectly distinct* in character, and differs also from the old red sandstone on which it may rest. I have also found some small fossil ribs in the sandstone, with fossil vegetables on a weather-worn surface—and, moreover, the leafy extremity of some large fronds in the same sandstone, very fine.—*Lett. to Edit.*, 18 *Aug.* 1836, *Sillim. Journ.*

Swarm of minute Insects near Macclesfield.

THE gloomy, cheerless, and almost wintry weather which prevailed throughout the greater part of September, was suddenly succeeded, on the morning of the 24th, by a gleam of delightful warmth, reminding us of the most enjoyable days of Summer. The wind blew softly from the south, the sun shone with the vigour of July, and the remnant of the preceding cold air lingering within our houses, presented a singular contrast with the genial zephyrs which welcomed the opener of a door or a window. Those who were early risers on the above Saturday morning must have observed the moist accumulation of vapour on the outside surface of their window-panes, consequent on this change of temperature.

Few are aware of the extraordinary and striking effects produced by these sudden atmospheric variations, affecting very intimately the economy of animal and vegetable life, and possibly, to a considerable degree, the inanimate world also, associated, as they are now proved to be, with electric or galvanic agencies. To what extent, or by what precise process, the above causes, separately or conjointly, operated in the production of the entomological phenomenon about to be noticed, occurring on Sunday the 25th, and partially on the Monday following, when the weather was cooler, and again on the Tuesday till rain fell, it is impossible to say; but to this sudden change was unquestionably attributable the unprecedented swarm of minute insects, which, literally speaking, almost filled the air on the days above mentioned. Over what probable area it extended, must be a matter of conjecture, but there is abundant evidence of this immense body occupying a space of at least twelve miles in length between Macclesfield and Knutsford alone, and about four in width, or in round numbers about fifty square miles. If we assign half a dozen of these insects to a square foot,—and we are sure this is a moderate computation—not less than seventeen [eight thousand four?] hundred millions (or about twenty [twelve?] times the number of the whole human population of the globe) of these tiny beings, each gifted with instinctive qualities, each furnished with a beautiful and perfect adaptation of physical structure for the purposes of its existence, were thus brought into visible life in the space of a few hours. Such an accession of vitality, as a matter of mere curiosity, cannot but be interesting, knowing so little as we do of the secret mode by which "the lord and giver of life" calls into action the elemental principles of animation, from man to the mollusca, "from the dust of the earth." But in the present instance the phenomenon assumes not a less interesting, but a somewhat more alarming, aspect, when it is ascertained that the mighty mass, for it was all one and the same, not only in genus but in species, was composed of a family of insects which, if unchecked and unlimited in their ravages, would produce a desolation in our woods and forests, fruits and flowers, more fatally and permanently destructive than any Egyptian plague on record. For be it known, that these countless myriads were neither more nor less than the representative body of a formidable and fearful *blight;* against which, where it once obtains firm footing, human means have hitherto been found useless and unavailing.

The insect belonged to the class *Hemiptera*, and to the genus *Aphis*, commonly known by the name of Plant Louse, and familiar to all, on the rose-trees, as those small wingless insects, which cluster together on the stems, usually just beneath the bud or the flowers, and on the larch, apple, and many other fruit-trees, as the downy, soft clammy mites, which abound more or less, according to the prevailing character of the season. There are about one hundred known genera of this pestiferous breed, of which about fifty, we are sorry to say, have found a legitimate place, as indigenous or naturalized (for some of the worst have been imported from America and elsewhere) in Great Britain.

The species which has led to these remarks, by its abrupt and multitudinous intrusion, is a beautiful subject for microscopic examination when exposed to strong solar light, for otherwise they appear to be nearly black. The wings, four in number, were perfectly transparent, rather iridiscent, and with few reticulations. The head and thorax black, the metothoracic scutum marked with two bright orange bands. The eyes globular and prominent. The antennæ filiform, or of equal thickness, consisting of eight or nine articulations, the terminal one rather attenuating at the point. From the jaws a proboscis projected, which bent down so closely as to be nearly hidden under the thorax, and was not easily seen. The legs were six in number, of a tawny colour, with the exception of the thicker part of the tibia, which was black; the remainder of the tibia, and the whole of the tarsus, being almost semi-transparent; the latter was also partially furnished with short bristles; at the extremity of the abdomen was a short tubular horn; the colour of the abdomen was of a pale yellowish-green and black, in lateral stripes. Without very correct plates, or cabinets of reference, it would be hazardous to assign the exact specific name, but in description, it allied itself so nearly with one too well known, *Aphis pruni*, or common plum-blight of the Spring, that it might be almost identified with that species.

Of all classes of insects, that of the Aphis assuredly presents the most singular and peculiar properties. While some are winged, others are not so, and this without distinction of age or sex. In the early part of the year they are viviparous, or producing their young alive; whereas, in the Autumn, they are oviparous, or layers of eggs, which remain throughout the Winter; but by a surprising aberration from the common laws of nature, it appears, that one impregnation of the female is sufficient for seven, certainly, and it is suspected of many more generations; that is to say, that the first female will lay eggs, productive of other females laying their eggs, and successively productive of seven or more broods; and when it is further known, that in five generations one single Aphis may thus be the parent of nearly six hundred million descendants, well may our foresters, nurserymen, and gardeners, tremble at the bare possibility of the stupendous influx we are now noticing, each carrying on its prolific capabilities without check or restraint, by which in the early part of next Spring, such an appalling pestilence may be turned loose to make a barren wilderness of our gardens and pleasure-grounds. But Nature—ever provident and circumspect—for the possible evil has provided various checks, each or all of which are ever at work in neutralizing the devastating effects which might otherwise ensue from similar causes.—S. *Analyst.*

Tobacco, a remedy for Arsenic.

About the year 1820, Miss Sophia Eastman, of Holles, New Hampshire, U. S., (now connected with the orphan asylum in Troy, New York,) fell into the mistake, so often committed, of eating a portion of arsenic which had been prepared for the destruction of rats. Painful symptoms soon led to inquiry; and her mistake was discovered. An elderly lady who was present, advised that she should be made to vomit as speedily as possible, and as she had always felt a perfect loathing for tobacco in every shape, it was supposed that this would at once effect the purpose. A pipe was used, but without producing any nausea. She next chewed a large portion of strong tobacco, and swallowed the juice, and that without even a sensation of disgust. A strong decoction was then made with hot water, of which she drank perhaps half a pint. Still there was neither nausea nor dizziness, nor did it operate at all either as an *emetic* or a *cathartic.* The painful sensations at her stomach, however, subsided, and she began to feel well. On the arrival of physicians, an emetic of blue vitriol was administered, which operated moderately once. One or two days after, there was a discharge of a dark-green colour, approaching to black. No ill consequence followed.

Another case occurred in the same place, a few years subsequent, in which arsenic was taken through mistake, by a sick person, and she employed tobacco with the like success. She, too, had always loathed the article, but now chewed it and swallowed the saliva, without producing sickness at the stomach. No emetic was administered in this case, nor any other remedy. Happy will it be for our race, should this insidious poison, now the slow death of so many, be employed only as an antagonist to those other deadly poisons, for which it may have been provided by the Creator, as a sure and speedy remedy.

The above facts I lately received from Dr. Eastman, of Holles, the father of Sophia, and from her sister, at whose house Sophia committed the mistake.—Emerson. 26 *May*, 1836. *Sillim. Jour.*

Ancient and Modern Physics compared.

The recent travels in Asia Minor of M. Texier, and his observations and measurements along the chain of the Taurus, are likely to be productive of some very interesting comparisons in the physics of ancient and modern times.

Though it has been demonstrated by the invariability of the length of the day, that the mean temperature of the earth has not varied for 2000 years, it is very far from being proved that the temperature of the surface, and particularly that the maxima and minima of it, have been constant. Observations by the ancients, of a nature that could be used in such comparisons, are very rare, and the problem has remained unsolved for want of the necessary elements. But now, with the assistance of M. Texier, we can discuss the accounts which Xenophon gives of the same localities during the *Retreat of the Ten Thousand.* Xenophon speaks of perpetual snows,—of wine frozen in the borachio or skin, and—of symptoms of somnolency and asphyxia, similar to those that Solander and his associates experienced in the South Seas. The notes, &c., of M. Texier on the Taurian chain, will enable modern *physiciens* to ascertain if the same natural phenomena now occur in the seasons correspondent to those in which Xenophon traversed those countries. M. Texier is said to have arrived during the summer at Trebizond, with large collections of geological and mineralogical specimens.—Libri.

A Contrast!—"Look here, on this picture, and on this."

NEWCASTLE.

STAYING lately in the neighbourhood of Newcastle-upon-Tyne, I repeatedly visited the Museum of the Natural History Society of that town, and I was much pleased with the collection, and the admirable state in which it is kept; but I was more especially gratified with the liberality of the subscribers in throwing the Museum open, without the necessity of an introduction, or any charge to their fellow-townsmen,—a facility of access scarcely to be expected, except in a national establishment like the British Museum. The museum of this society was formerly opened to all classes in an evening, when it was lighted up for the occasion; but the visiters who availed themselves of the privilege were so numerous, that it was impossible for them to inspect the collection with advantage. The committee, in consequence, was under the necessity of altering their mode of admission; they now issue a certain number of tickets each night, which are sent to the workmen of the different factories in the neighbourhood, in rotation, for the admission of the holder and his family, or to such persons as make previous application at the institution; a plan which has been found to give general satisfaction.

The anniversary meeting occurred during my stay in Newcastle, and it is characteristic of the liberality of the subscribers, that one of its members rose and inquired if the council had taken into consideration how *increased* facilities could be given for the admission of the public to the museum. The collection of the Antiquarian Society, (which contains many very interesting specimens of art, deposited in another part of the building,) is, also, in a like manner, open gratuitously to the inspection of the public; and I sincerely wish this liberality was more generally displayed in similar societies, as I firmly believe that, if such a plan were adopted,

MANCHESTER.

MANY institutions are open by an order from, or an introduction by, a subscriber; but in most of these I have observed that admission is granted on application to the curator of the institution. At Liverpool, Edinburgh, Glasgow, and Worcester, the museums are open to any visiter on the payment of one shilling, as in some of the proprietary museums which were formerly exhibited in the metropolis. But the Natural History Society of Manchester, as far as I am aware, stands alone for the exclusive conduct of its supporters. In my situation in the British Museum, I have had several requests made to me from residents in Manchester, to compare specimens of British birds, or other objects of the kind, with similar specimens in the collection under my care; for the artisans of Manchester, like the weavers of Spitalfields, appear to be very fond of the various branches of Natural History, British Ornithology and Entomology in particular. Some of these persons, who generally appeared to be workmen, stated, as an excuse for what they considered an intrusion on my time, that they, or their friends, could not make the comparisons in their own town, though its Museum contained a good collection of British birds, &c., as no resident was allowed to visit the museum but members of the society, and that the members were elected by ballot, so that there was no hopes of their being admitted, even if they could afford to pay the subscription. I did not pay much attention to their complaints, and they entirely escaped my recollection.

Being near Manchester last year, I passed through the town, on my road to London, for the purpose of examining the museum. On asking my way to it of a gentleman whom I happened to meet in the street, he inquired if I had an order, as I could not otherwise be admitted. I stated my occupation, and observed that I had visited most of the British and Continental museums, and had never been refused admission; I therefore did not imagine an order would be requisite. He assured me it was indispensable, and kindly offered to give me and my friends a personal introduction. Even with these preliminaries, I must say that I was not preit would have the effect of increasing the funds of the institution, from the number of persons who would take an interest in its prosperity; and the subscribers would have the gratification of knowing they were promoting the spread of knowledge, good taste, and feeling, among their fellow townspeople. This society, besides setting so good an example to other institutions, has distinguished itself by the energy and scientific knowledge of many of its members, who have published papers in their *Transactions* which may rank with productions on similar subjects in the *Transactions* of our metropolitan societies.——GRAY, *Analyst*.

Minute Registry of Storms desirable.

IT is hoped, that in future notices and reports of violent storms, more attention will be given to specific dates and location, and also to the direction and changes of the wind, all which may be expressed in the most summary manner; and the facts, when once recorded, are for ever available, in tracing the progress and character of such storms. It seems desirable also, that the general route and character of European storms should be investigated by those whose local position, and means of information, best qualify them for the task.—REDFIELD, *Sillim. Journ.*

Ascent of Hecla.

MOUNT HECLA, the volcano of Iceland, was ascended last year by some of the French scientific expedition. It was entirely covered with deep snow. This mountain is 5000 feet high, and is volcanic from *top to bottom*.

pared to see the following inscription painted in large letters on a black board, hanging from the door; on reading which, the complaints of my Manchester visitors immediately occurred to my memory; it runs thus:—

"I.—No gentleman residing within three miles of Manchester, not being a subscriber, can be admitted.

"II.—No visiter can be admitted without a personal introduction by a proprietor, or by a written order, with the name of the party to be introduced.

"III.—No servants or young children can be admitted."

I had the pleasure of meeting one of the leading members of the society in the building, and had some conversation with him on the subject of the board, when he informed me that it was considered necessary to make such regulations, otherwise they would not be able to support the institution, as many persons who are now subscribers would come as visiters. I must say that I doubt the policy, and regret the feeling which should dictate such regulations, especially the last, as I should have thought the admission of children, with a view of imparting a taste for the beauties of Nature in the rising generation, would have been one of the most ardent wishes of the subscribers. —GRAY, *Analyst*.

Central Society of Education.

THIS title has been lately adopted by a body of enlightened and influential persons, at the head of which, as President, is Lord Denman*.

The object of the society, it is stated, is to collect, to classify, and to diffuse information concerning the education of all classes, in every department, in order to learn by what means individuals may be best fitted, in health, in mind, and in morals, to fill the stations which they are destined to occupy in society.

For the attainment of this object, the society proposes to obtain, and from time to time, (probably periodically) to publish, 1st. Accounts of systems of education already established, whether in this country or abroad; 2nd. Discussions of the value of various branches and means of education; 3rd. Accounts of books, maps, models, and other aids of education.

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* It should not be forgotten, that this eminent man had the courage to repeat in the House of Lords the important maxim, "that for a state to have the right to punish, it must educate."

Notwithstanding the importance which is now generally attached to education, and the opinion which many entertain, that in this country it is in many parts imperfect, and does not tend to fit men for the fulfilment of their peculiar duties; there is not in the metropolis a single association, and scarcely a work devoted to recording and suggesting improvements in this the most essential of all sciences and arts.

The labours of the committee will divide themselves under five principal heads:

1. Primary or Elementary Education.
2. Secondary Education.
3. Superior or University Education.
4. Special or Professional Education.
5. Supplementary Education.

If their materials are as extensive as they hope, the committee will issue periodically separate publications in each of these departments.

The society being necessarily composed of members of various religious denominations, it is obvious that they can have no sectarian objects in view, whenever they may admit (as they intend to do) into their publications, articles which shall discuss the best modes of uniting intellectual and religious education, without offending the opinions of any class of Christians.

The subscription is £1 annually, and *it has been determined that no expense shall at any time be incurred beyond the funds actually in hand.*

From the following statement in the *Analyst*, Jan., 1837, it appears this Society has began its labours in a most exemplary manner:—

"The first labours of the Society have, we have reason to know, been directed to the collection of facts respecting the actual condition of the poorer population of a district of the metropolis. This it is now occupied in examining with the greatest accuracy, going from street to street, from house to house: not contenting itself with ascertaining merely the literary attainments of the children, but observing all those circumstances over which an individual has a control, and which, consequently, indicate with certainty *the education he has received* and *the education he is in want of.* The consequences of this inquiry will, we are led to believe, be most important, as it will enable the Society to state precisely, in numbers, the exact quantity of particulars upon which its affirmations with regard to any general fact is grounded. The condition of the people, in a moral, intellectual, and physical point of view, in our large towns, is such, that any statement with regard to it, other than such as the Central Society of Education is now qualifying itself to make, would be disbelieved. Those who only ride or drive through the wide and clear streets of our metropolis, could not credit that the state of the mass is such as it is. An intelligent foreigner who visited England some time back said that, for his part, he could never see the poor, nor where they lived."

We hope that the philanthropic and intelligent individuals who have been the means of embodying this society, will excite and direct its energies with unceasing activity. A union of a similar nature took place at the close of the Bristol session of the British Association: this included some of the names which we now see on the Committee of Management of the Central Society. Will this nucleus slumber until awakened in Liverpool by the next trump of the greater body, from which it made an effort to shoot off?

Chemical Action of the Solar Rays through Fluid Prisms.

PROF. HESSLER, of Gratz, has found that the action of the Solar spectrum on paper which has been moistened with a solution of gum, and sprinkled with chloride of silver, varied with the nature of the prism. The action differed both in the extent and rapidity of its effect, and also in the point of the spectrum where it attained its maximum. It was nearly instantaneous with a prism of water or spirits of wine; occurred in the course of twelve or thirteen minutes with the oils of turpentine and cassia; in two minutes and three seconds with flint-glass, and one minute and five seconds with crown-glass. The maximum chemical effect with spirits of wine, was obtained in the violet near the blue; with water, in the violet; with oil of cassia, twenty-three lines outside of the violet.

Grand French Mathematical Prize deferred a Third Time!

In December, 1805, the *Académie des Sciences* stated that, after having twice unsuccessfully presented the question of the resistance of water as the subject for this prize, they had withdrawn it from competition, but that new circumstances had arisen which determined them, once more, to call the attention of experimenters and of geometricians to this important subject of inquiry.

These new circumstances were principally the unforeseen advantages which had been found in England to follow the practice of tracking canal-boats with very high velocities, and which had opened an immense field of research for the enrichment of science and the improvement of internal navigation. The various points of view in which this great problem might be regarded, were too apparent to render it necessary to point them out to the candidates.

The reduction to a vacuum of pendulum-observations made in the atmosphere were, but a very short time ago, still calculated by an inaccurate method, although the long-known experiments of Dubuat ought to have pointed out the road to the true solution. The labours of Messrs. Bessel and Baily, and the analytical researches of a member of the *Académie*, notwithstanding their great interest, have not entirely exhausted the subject. The *Académie* would, therefore, without making it an express condition, be gratified by seeing candidates endeavour to illustrate so much of the problem of the resistance of media, taken in this point of view, as may still require explanation.

In December last, the *Académie* had again the mortification (in which the scientific world will deeply participate,) to receive the following Report from the Commission to whom the Subject of the Grand Mathematical Prize offered a third time in 1835 had been referred:

"The Commission, after having examined the Memoirs addressed to the *Académie* for the Grand Prize of Mathematics in 1836, have decided, that there is not yet any ground for granting the Prize, but convinced, that the want of time, the difficulty, and the too great extent, of the subject, have prevented authors giving to their experimental or theoretical researches the necessary excellence, the Commission proposes that the question be deferred to the approaching competition of 1838, recommending that the *Académie* should not require from the candidates the discussion of the whole of the questions in the original programmes. The Commission would be interested to see the candidates confine themselves to the examination of such of the questions as may appear to them the most susceptible of solution by direct experiment, and of being carried to that degree of perfection which alone can be useful to science."

The above proposition was adopted by the *Académie*. It is therefore hoped, that the thing will be decreed in 1838. Agreeably to the previous conditions, works or memoirs intended for this prize, must be delivered without fail to the secretary of the *Institut*, before the 1st of July, 1838. Candidates may either send in their names, or enclose them in a sealed envelope.

A National Slander refuted.

It was with the greatest pleasure that I heard it stated in the Report of the Newcastle-on-Tyne Society, that, notwithstanding articles of great value were exposed on the cases without any cover, they had never lost a single specimen, nor had any part of the collection been injured by the visiters. This account quite agrees with my own experience in the British Museum, where there have been occasionally more than six thousand visiters in a single day. During the last twelve or thirteen years I have been in that institution (and the greater part of this time I have had the immediate superintendence of the zoological part of the collection), I do not recollect a single instance of wilful injury, and, indeed, hardly of carelessness, on the part of the visiters, though now and then a pane of glass may be cracked; but that is scarcely to be avoided from the frequently crowded state of the rooms, with glass cases in every direction. From my experience in the British Museum, and in other situations, I think that the English public have been most unjustly abused in this respect; partly arising from that delight which the English have in complaining of their countrymen, and praising foreigners at their expense, and partly by designing persons, who have profited by places being kept from public view, except on the payment of fees. For example: I do not think (though the accusation has been repeatedly made) that the English are more inclined to write on walls than our continental neighbours*, although they have not the constant dread of the *surveillance* of the police, which the French appear always to have before their eyes. In those places where it can be done with little chance of detection—as in the passages of the Courts of Justice, in Paris—I have seen the walls much disfigured by writing in charcoal instead of chalk; the French hand in which they were written, and the names, at once showing it was the work of natives.

The French police interfere in the most trifling cases, and their conduct in this respect must afford much amusement to an observant Englishman. As an instance of their severity, I have seen visiters to the Museum of the Garden of Plants rebuked, in no very measured terms, for merely accidentally touching the glass of the cases with their fingers in pointing to a bird: and for any infraction of the rules of that institution they are immediately arrested by the military guard who have the care of the rooms during the public exhibition. I was once arrested myself, because Mrs. Gray was carrying in her hand a roll, consisting of half a sheet of letter-paper, on which we had been making some notes before the gallery was opened to the public, but which we should have put out of sight previous to the visiters being admitted. They led us, with a guard of three soldiers, along the galleries, to the guardian of the collection, who immediately set us at liberty.

In other parts of the continent, as in Switzerland, where the inhabitants are not under the *surveillance* of the police, the walls are as much disfigured by writing as in England; and I need only instance the chapel of William Tell. This remnant of barbarism, therefore, which has been called by some "English taste," is not peculiar to our country, and I am inclined to believe that a great improvement in this respect is taking place amongst the English; indeed, I have no doubt, as the education of the people advances, it will rapidly disappear. I feel assured that the best and most speedy way to eradicate the evil will be to adopt, in the various local institutions, the liberal example of the Natural History Society of Newcastle-on-Tyne, as the means best calculated to impart a taste for the beauties of the creation among the people; and if the picture-galleries, churches, cathedrals, and other buildings containing works of art in the country, were freely opened to their inspection, it would have the effect of giving them a taste for the fine arts. I think the exemplary behaviour of the visiters in the British Museum, and in the Museum of the Newcastle-on-Tyne Society, fully justifies a similar trial in other places.—GRAY, *Analyst.*

Effects of Electricity by Contact.

THE following is a summary communicated by Hr. von Humboldt of the results to which Hr. Karsten of Berlin has been led by his recent investigations.

1. Metals, and probably all solid bodies, become positively electrified when immersed in fluids; the fluids are negative.

2. A solid, partially immersed in a fluid, acquires electric polarity; the part not immersed being negative, and the other positive.

3. Solid bodies differ greatly in their electro-motive power in regard to the same fluid, and this difference is the true cause of the electric, chemical, and magnetic action in the galvanic circuit.

4. If two solid electro-motors, of different electro-motive power, are immersed in the same fluid *without being in contact with each other*, the weaker electro-motor receives a polarity opposite to that of the stronger, and becomes consequently negatively electric.

5. The part of the weaker electro-motor not immersed, exhibits opposite electricity to that which is immersed, that is to say, it is positive.

6. The electro-motive action of a fluid depends on the property of its being reduced, by two solid electro-motors of dissimilar power, to such a state, that the solid electro-motors receive from it opposite electricities. In general, all fluids which are bad conductors of electricity possess this property, but not those which are good conductors, (mercury, metals in fusion, &c.,) nor those which have no conducting power, (oils, &c.) The intensity, however, of the electro-motive power of the fluids does not depend on the more or less perfect

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* I never recollect to have seen an instance of it in the part of the British Museum dedicated to the Natural History Collection.

conductibility only, but on other relations not fully known at present.

7. The electro-motive effects of two metals which form a closed circuit in the same fluid, depend on the continual excitement and neutralization of opposite electricities in the fluid. They are generated by the electro-motive action of the two electro-motors on the fluid; are augmented by the action of the stronger on the weaker; and are accelerated by the close contact of two solid electro-motors, when these are good conductors.

8. The chemical changes in the fluid, it is true, have a relation with the neutralization of the two electricities produced by the solid elements of the circuit, but these chemical changes and the neutralization have not the mutual relation of cause and effect.

9. In the *system* of circuits composing the voltaic pile, the opposite electricities are completely neutralized by the solid elements of each circuit, that is, by the pairs of plates, and there is no electric current from one to the other.

Cause of Decrepitation.

MANY bodies if suddenly submitted to the action of a high temperature, fly into pieces with a crackling noise. It is to this phenomenon, that the name of decrepitation has been given: most authors who have treated upon this property, have generally ascribed it to the presence of water in the body, or that the decomposition evolved aëriform products, which violently separated the particles. It is, however, remarkable, that the greater number of bodies which decrepitate, are really anhydrous and fixed, such as sulphate of potash, sulphate of barytes, chloride of sodium, &c. To explain this species of anomaly, or rather inaccuracy in the explanation of facts, they suppose, that although these bodies do not contain water in a state of *combination*, they hold it *interposed* between their constituent parts. As this opinion did not appear to me to have any foundation, I dried, at low temperatures, and by various means, several fixed and anhydrous bodies, susceptible of decrepitation, and I found, that although this desiccation was as complete as possible, they would still decrepitate when suddenly heated.

I had proceeded so far, when, eight years ago, I observed that argillaceous schist mixed with coal decrepitated strongly when thrown into a heated oven, and that the noise was in proportion to the quantity of surface exposed. As argillaceous schist was of a well-defined laminated structure, I was led to examine if anhydrous bodies susceptible of decrepitation did not possess some similar or analogous property, and I found, in fact, that no bodies decrepitated, unless they did possess crystalline structure, and had a clean and easy cleavage. This property of splitting cleanly with large surfaces, renders the fact of their decrepitation of easy explanation. For the substances which decrepitate being of that class which are considered bad conductors of heat, it follows that their external parts are the first to be heated, and that their dilatation forces them to separate from adjoining parts which have not attained the same temperature, and which separation is facilitated by the property of cleavage which they possess.

There are substances which in their decomposition give out volatile products on being heated: in these cases, it is difficult to decide, whether their decrepitation is owing to an unequal dilatation of their parts, or to the repulsive action of the volatile products. However, as they nearly all possess a crystalline structure, and, at least, one direction of easy cleavage, I am led to believe that this structure is often the only cause, for they really decrepitate before undergoing any apparent decomposition, as the cyanide of mercury, &c.

I ought, however, to add, that substances in which crystalline structure is entirely absent, will decrepitate when they are not perfectly dry; such are plastic clay and argillaceous schist.

It results from the preceding, that bodies which decrepitate when heated, may be divided into two principal classes, namely,

1. Fixed bodies.
2. Bodies which give out aëriform products.

In the first class may be placed

Sulphate of barytes,
Sulphate of strontium,
Sulphate of potash,
Fluoride of calcium,
Chloride of potassium,
Chloride of sodium,
Bromide of sodium,
Bromide of potassium,
Iodide of potassium,
Galena, cubic or laminated.

In the class of bodies which decompose in giving out aëriform products at a high temperature, there may be distinguished, 1st, the *anhydrates*,—

Nitrate of barytes,
Nitrate of lead,
Carbonate of lime, rhomboidal,
Cyanide of mercury;

2nd., the *hydrates*,—

Sulphate of lime, laminated,
Acetate of copper,
Cream of tartar,
Ferrocyanate of potassium, yellow.

Substances which contain, in a state of combination, the elements of water in a large quantity, do not really decrepitate, unless they are susceptible of cleavage, such as carbonate of soda, sulphate of soda, hydro-sulphate of magnesia, &c.

Cleavage is, therefore, a condition of decrepitation, at least as necessary as the presence of water, or of its elements. —BAUDRIMONT.

St. James's Park Ornithological Society.

HERE is a Society that "has no privileges to claim or to offer, except those of rendering service to science, and contributing to the amusements and information of the public." It addresses itself to "all lovers of the beauty of Nature, to all who can appreciate the charm which the feathered tribe, that most beautiful portion of the animal creation, are capable of lending to ornamental water."

The object is, to obtain,—to preserve, as far as possible, in a state of nature, and—to bring under the notice of all classes and ages, on the waters of the Parks, (beginning with that of St. James',) a complete collection of the British species of the genus *Anas* of Linnæus, from the Swan (who has held for ages this sole privilege,) to the smallest of the Ducks, and ultimately to include specimens of every species of hardy aquatic birds;—Waders, Swimmers, and Divers.

The idea is a most happy one, and, we predict, will soon be realized. To further the views of the Society, we give a list of their present stock and their desiderata.

List of Birds already in the Society's possession:

Swans:—Domestic.

Geese:—Chinese, Bean, White-fronted or Laughing, Canada, Egyptian, *Bernacle*.

Ducks:—Summer, Shieldrake, Muscovy, Wild, Hookbill, Shoveller, Pintail, Wigeon, Teal, Pochard or Dunbird, Tufted, Call, Buenos Ayres.

Various:—Spoonbill, Night Heron, White Stork, Gull, Coot, Little Grebe, Moorhen.

DESIDERATA.

Swans;—Hooper, Bewick's, Polish, Black, Trumpeter.

Geese;—Gray-lag, Brent, Hutchins', Red-breasted, Spur-winged, Sandwich Islands, Cereopsis.

Ducks;—Eider, Gadwall, Ruddy, Bimaculated, King, Western, Velvet, Scoter, Surf Scoter, Red-crested, Castaneous or White-eyed, Scaup, Harlequin, Long-tailed, Golden Eye, Summer, Shoveller, Red-billed Whistling, Black-bellied Whistling, Buffel-headed, Canvas-back, Dusky, Pied, Mandarin Teal, Japanese Teal.

Divers;—Great Northern, Black-throated, Red-throated.

Grebes;—Crested Grebe, Eared, Red-necked, Little, Carolina or Pied-bill, Horned, Goosander, Red-breasted Merganser, Hooded Merganser, Smew, Gannet, Bitterns, Herons, Cranes, Roseate Spoonbill.

Patent-Law Grievance. No. XI.

THE inventors of this country, and the introducers of inventions of other countries into this, were obliged to pay down to the attorney-general and other agents, &c., of the Government during the past year, above £42,000. *What did the attorney-general, &c., in return to them for this vast and oppressive extortion?*

The penalties inflicted on the inventive genius of Britain during the *present* year, up to the 25th ult., in the shape of government stamps and fees on patents, amount to more than £3000!

N.B. This sum has been paid in *ready money* on taking the first steps, and as many of the inventors are poor men, (*Operatives*,) and a great many others of them persons to whom it would be very inconvenient to pay at least £100 down, they have been obliged to go into debt, or mortgage or dispose of their inventions, either wholly or in part, &c.

Will Messrs. Mackinnon, Brewster, Babbage, &c.,—friends of the poor inventor,—be early in the field during the parliamentary session which began yesterday?

NEW PATENTS. 1836—1837.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second that on or before which the Specification must be delivered and enrolled.——The abbreviation *For. Comm.*, signifies that the invention, &c., is "a communication from a foreigner residing abroad."

December *cont.*

296. Hamer Stansfeld, Leeds, *York*, Merchant; for an application to certain machinery of a tappet and lever action, to produce a vertical or horizontal movement through the medium of ropes or bands, working over, under, or round pullies. Dec. 30.—June 30. *For. Comm.*

Total, December...32.

January, 1837.

1. William Cooper, Picardy-pl., *Edinb.*, Glass Merchant; for an improved method of executing ornaments, devices, colours, or stains on glass. Jan. 10.—July 10.

2. Robert Griffiths, Smithwick, *Warw.*, Machine Maker; and Samuel Evers, Cradley Iron Works, *Staff.*, Iron Manufacturer; for improvements in the manufacture of bars or nuts for screws. Jan. 11.—July 11.

3. Henry Adcock, Birmingham, *Warw.*, Engineer; for improvements in the construction of the furnaces employed in the reduction of iron ores and other metallic ores, as also in some of the processes of the iron manufacture of other metals, such furnaces being applicable to other purposes. Jan. 11.—July 11.

4. James Gardner, Banbury, *Oxf.*, Ironmonger; for improvements in cutting Swedish and other turnips, mangel-wurzel, and other roots used as food for sheep, horned cattle, and other animals. Jan. 11. —July 11.

5. Charles Sheridan, Ironmonger-lane, *Lond.*, Chemist; for improvements in the manufacture of soda. Jan. 11.—July 11.

6. John Paul Neumann, Gt. Tower-st., *Lond.*, Prussiate of Potash maker; for improvements in the manufacture of prussiate of potash, and in prussiate of soda. Jan. 11.—July 11. *For. Comm.*

7. George Goodlet, Leith, *Edinb.*, Merchant; for an improved mode of distilling spirits from wash and other articles, also applicable to general purposes of rectifying, boiling, and evaporating or concentrating. Jan. 11.—July 11.

8. Francis Gybbon Spilsbury, Newman-st., *Middx.*, Engineer; and William Maugham, Newport-st., Lambeth, *Surry*, Chemist; for improvements in the manufacture of carbonate of soda. Jan. 11.—July 11.

9. John Macneill, Parliament-street, *Middx.*, Civil Engineer; for improvements in making or mending turnpike or common roads. Jan. 11.—July 11.

10. James Braby, Duke-st., Lambeth, *Surr.*, Wheelwright and Coachmaker; for improvements in the construction of carriages. Jan. 11.—July 11.

11. Robert Sewell, Carrington, *Nott.*, Lace Manufacturer; for improvements in the manufacture of white-lead. Jan. 11. —July 11.

12. Charles Thornton Coathupe, Wraxall, *Somer.*, Glass Manufacturer; for improvements in the manufacture of certain descriptions of glass. Jan. 11.—July 11.

13. John Gall, Aberdeen, *Scotl.*, Carpenter and Builder; for an improved mode of priming fire-arms, applicable to percussion locks. Jan. 17.—July 17.

14. Arthur Dunn, Nelson-st., City-rd., *Middx.*, Manufacturing Chemist; for an improved mode of dissolving siliceous matter, and compounds of silica, and of manufacturing soap. Jan. 17.—July 17.

15. William Gossage, Stoke Prior, *Worc.*, Chemist; for improvements in manufacturing oxide of lead, applicable to making paints, and to other purposes; also, improvements in the process of bleaching and purifying oils, suitable for mixing paints, and other oils and fatty matters. Jan. 19. —July 19.

16. John Murray, Fitzroy-sq., *Middx.*, Gent.; for improvements in the construction of carriages. Jan. 19.—July 19.

17. Moses Poole, Lincoln's-Inn, *Middx.*, Gent.; for improvements in ordnance and other fire-arms. Jan. 19.—July 19. *For. Comm.*

18. Henry Needham Scrope Shrapnell, Bayswater-terr., *Middx.*, Esq.; for improvements on snuffers. Jan. 19.—July 19.

19. William Stedman Gillett, Guildford-st., *Middx.*, Gent.; for improvements in trimming and facilitating the progress of vessels in water. Jan. 21.—July 21.

20. Julius Oliver, Castle-st., Falcon-sq., *Lond.*, Gent.; for an improvement in the filters employed in sugar-refining. Jan. 24.—July 24. *For. Comm.*

21. Joshua Cuttel, Hollingforth, *York*, Woollen Manufacturer; for improvements in producing slubbings of, and in spinning wool. Jan. 26.—July 26. For. Comm.

other than an aqueous cause of terrestrial fluidity; according to them, all the diversified substances of which the globe is composed were originally dissolved in a liquid, and the solid parts have been formed by deposition or precipitation. The *Plutonists* on their side reject the idea of such a solvent: they maintain that the fluidity of the constituent principles of the globe was the consequence of an enormous temperature, and that the surface became solid by cooling. These two schools, I might have said *sects*, so great was the acrimony they manifested, attacked each other with inconclusive arguments borrowed from geological phenomena, only calculated to leave all dispassionate inquirers in a state of scepticism on the subject. One effectual method of putting an end to the debate obviously was, to examine whether beneath the surface there still existed traces indicative of that original heat insisted on by the Plutonists; and to this question *physiciens* and mathematicians, by their combined labours, have succeeded in giving a satisfactory answer.

On all parts of the earth, when we penetrate beyond a certain depth, the thermometer ceases to undergo either diurnal or annual fluctuations; in these recesses it indicates at all times the same constant degree, and even fraction of a degree, during the whole year, for any number of successive years:—such is the fact; how far does theory explain it?

Let us imagine the earth to have received at any time all its heat from the sun: calculation founded on this hypothesis proves, 1st, that at a certain depth below the surface the temperature would be invariable; and, 2dly, that this *solar* temperature of the interior of the globe would vary with the latitude: so far both theory and observation agree; but according to the former, the constant temperature of the terrestrial strata in each climate would be the same at all depths, short of those which might bear any considerable proportion to the earth's radius. Now it is well known that this is not the real case: observations made in a multitude of mines, and on the temperature of springs, the waters of which are known to arise from different depths, all concur in indicating an increase in temperature of one degree C. (0°·6 F.) for each successive twenty or thirty yards to which we penetrate. When an hypothesis leads to conclusions so completely at variance with fact, it must at once be discarded as erroneous; hence, then, we may conclude, that *the phenomena of the temperature of the strata of the globe cannot be attributed to solar action only.*

This solar action once set aside, the only cause which can be assigned for the gradual increase of temperature everywhere observed on penetrating below the surface of the globe, is an original source of heat: the earth, as the disciples of the Plutonian school, as well as Descartes and Leibnitz, asserted, but without satisfactory evidence, is definitively become a sun incrusted over, the high temperature of which may be confidently appealed to in explanation of geological phenomena.

Do any data exist for ascertaining for how many ages the earth has been cooling?

The present state of science enables us to deduce numerical values from many of the formulæ derived from the theory of heat, while the solutions of other problems relating to this subject are still confined to general analytical expressions: among these latter there is one for the secular refrigeration of the earth, and the *number* of ages elapsed since the commencement of that refrigeration enters into the formula, on the assumption, however, that the temperature was common to the whole mass of the globe at that epoch. If this number were given, we might assign the numerical value of the secular loss of heat; and, reciprocally, if this latter quantity were once known, we could readily ascertain the period at which the cooling commenced. The question of the antiquity of the globe, so long and so vigorously disputed, including even the period of its incandescence, would be thus reduced to the determination of a thermometrical variation; but it would require centuries to answer this, on account of the minuteness of the quantity.

Proof deduced from the moon's motion, that the general temperature of the mass of the globe has not varied the tenth part of a degree in the last two thousand years.

Having concluded that the earth was originally incandescent, that its present solid crust has been formed by cooling, and having shown that its heat, even at moderate depths, is still enormous; it necessarily follows from this last circumstance that it must continue to cool, and it is only the quantity that can be a subject of doubt. Now the lunar motion furnishes a proof that the mean temperature of the earth, considered in its whole mass, and not at its surface only, has not decreased the hundredth part of a degree in the last two thousand years.

In proposing to explain how two phenomena, apparently so heterogeneous as the temperature of the earth and the motions of her satellite, can have any mutual connexion, I trust that I have not deceived myself in considering this problem as one, the solution of which may be familiarly explained, without the use of calculations.

Suppose a heavy mass were adapted to each radius of a wheel, so as to admit of being placed at any part of its length at pleasure; and let these masses, at first, be set near to the axle; it would require a certain force to communicate a velocity of rotation to this wheel, say of one revolution per second. If the masses be then conceived moved to the outer extremities of the radii, the total weight of the apparatus will not thereby be affected, yet it would obviously require a greater force to make the whole rotate with the same velocity of one revolution per second, as before. Since it appears, then, that in order to turn a mass of a given weight with a given velocity, a greater force will be requisite, in proportion as the elements of which the total weight is composed are the more removed from the axis of rotation; it follows, that with a constant force, the rotatory velocity of the mass will be retarded in proportion as its different particles are more distant from the axis.

As all bodies are dilated by heat, and contracted by a diminution of it, such a mass as we have alluded to will expand in proportion to its increased temperature, or its component particles will be further removed from the axis of rotation; and a diminution of temperature would produce a contrary effect. Hence, under the influence of the same force, a given mass will revolve with an increasing or diminishing velocity, according as its temperature was becoming lower or higher*.

All that has been cited of a flat wheel will evidently apply to a mass of any figure whatever, as, for example, a spheroid such as our earth, which rotates on an axis by virtue of an original impulse. If its dimensions are by any means augmented, its velocity of rotation will diminish, or the sphere will require a longer time to make one revolution. If, on the contrary, the spheroid contracts, its velocity will be accelerated; it will occupy less time to make a revolution.

The materials of which the earth is composed expand by heat, and contract by cold; those who believe that the earth is cooling, must therefore acknowledge that its radius is diminishing, or that its volume is becoming smaller and smaller. But we have just shown, that this volume cannot diminish without a proportional increase in the velocity of rotation; hence the question, whether the temperature of the globe, two thousand years ago, was the same as it is now, is equivalent to this,—Did the earth occupy precisely the same time to accomplish a rotation, two thousand years ago, that it now does? Under the first form, the solution of the problem seemed absolutely to require thermometrical observations, of which the ancients had no idea; while, on the contrary, we find in the astronomical ones they have bequeathed to us, means for ascertaining whether the time of rotation has remained constant.

The duration of this revolution is an unit of time which is, and ever has been, made use of by astronomers of all ages; it is termed a sidereal day: the question as to temperature may hence be transformed into one on the measurement of time, because the ancients were unacquainted with the thermometer. If it be demanded, what we have gained by the change, since pendulum-clocks were equally unknown to antiquity, or, at least, since no such machine has been transmitted to us, it may be replied, that we have means for determining the length of a sidereal day, two thousand years ago, infinitely preferable to any machine, the identity of which may be disputable, and which would inevitably have been deteriorated by time.

The *proper motion* of the moon has attracted attention in all ages, and man has always been anxious to measure her velocity; but the measurement of velocity implies the choice of an unit of time,—this we may assume to be the sidereal day. To obviate any objection against the choice of this unit in measuring the moon's velocity, the duration of this day, or, which is the same thing, that of the earth's rotation, must be independent of the velocity of our satellite; that this is the case is manifest, since, if the earth were all at once to cease rotating, the moon would not the less continue to revolve in her orbit.

The school of Alexandria has left us observations from which we can deduce the mean distance moved through by the moon during a sidereal day with the greatest exactitude. The Arabian astronomers have furnished us with the same data for the time of the caliphs; and there is not a single catalogue of modern observations, in which the moon's mean motion during a sidereal day is not given. Now, the arc passed over by our satellite in that unit of time, is found to be precisely the same, whether deduced from the Grecian, the Arabian, or from recent observations*.

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* M. Arago cites as an illustration the familiar example of the compensation and *common balance*-wheels of chronometers and ordinary time-pieces; we need do no more than allude to them here.—Trans.

This important result involves the answer to the proposed question, as may be proved in a few words. The Alexandrian astronomer determined the length of his sidereal day, or the time of the earth's rotation, by direct observation, and noted down the arc through which the moon moved during that period: the same mode of proceeding was adopted by the Arabian, and is still pursued by the modern astronomer. Thus each reckoned according to the sidereal day *of his epoch;* and since we have shown that the moon always moves with the same mean velocity, the arc she moves over must be directly proportional to the time during which her motion is observed. If the sidereal day had been longer in the time of Hipparchus than it now is, the Grecian astronomer would have observed the moon during a longer period of time than the modern astronomer, and the daily motion of our satellite would have been found greater at the former than at the present time, or her velocity would appear to us to have *decreased.* But the arc passed through in one day has been precisely the same at all three periods; hence it may be asserted, that the sidereal day has always designated an equal space of time; and again, since the sidereal day and the time of the earth's rotation are equivalent expressions, the velocity of that rotation has remained constant for two thousand years; therefore, the earth's volume has not changed, and its temperature must have been likewise constant during that period, since any change in temperature would have effected a proportional one in volume. It now only remains to illustrate the subject numerically, to point out the precision of which the method is susceptible.

Let us suppose the mean temperature of each radius of the globe to have decreased one degree C. (1°·6F.) in two thousand years, and assume the average dilatation by heat of the substances composing the mass, to be the same as that of glass, that is to say, about $\frac{1}{100000}$, for each degree; a diminution in temperature of one degree, would consequently have occasioned one in the dimensions of the spheroid of $\frac{1}{100000}$, which, according to dynamical theory, would produce an acceleration in its velocity of rotation, of $\frac{1}{50000}$ which would shorten the sidereal day by 1"·7. But the observations of the moon alluded to above, prove that, since the time of Hipparchus, the sidereal day has not varied the $\frac{1}{100}$ *of a*

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* If we make use of the *unreduced* observations, the arcs passed through by the moon, at these three distant epochs, would *not* be equal: for in fact, ever since the time of the Chaldeans, the moon's velocity has been continually augmenting. But this increase is one result of what are called *perturbations*, and arises from a diminution in the eccentricity of the earth's orbit. When this eccentricity, which has hitherto been diminishing, shall begin to argument again, the moon's velocity will gradually be *retarded*, in the same degree as it had previously been accelerated, and so alternately, after the lapse of immense equal periods of time. Hence the constant velocity alluded to in the text, only appears after correcting the observations for the perturbations caused in the moon's motions by changes in that of the earth, and by other causes. When, therefore, it was stated that the velocity of the moon was independent of that of the earth, it was only of our diurnal *rotation* that I spoke.

*second**, a quantity 170 times less than $1''\cdot7$. Hence the change of temperature in the terrestrial radius assumed above is 170 times greater than the observations on the length of the sidereal day admit of its having been; and we may conclude, that the mean temperature of *the general mass of the earth* has not varied $\frac{1}{170}$ of a degree in two thousand years.

Does the primitive heat of the globe, which is still so perceptible at a certain depth, contribute in any sensible degree to the present temperature of the surface?

It was computed by Mairan, Buffon, and Bailly, that in France, the heat which escaped from the interior of the earth was 29 times as great in summer, and 400 times as great in winter, as that which was received from the sun; that is, according to this estimate, the heat we derive from the planet which illuminates us, is but a small part of that by which we benefit. This idea has been eloquently expatiated on in the *Mémoires de l'Académie*, in the *Epoques de Buffon*, and in Bailly's Letters to Voltaire, *on the Origin of the Sciences, and on the ancient Atlantis*; but the ingenious romance of which it furnishes the basis, has disappeared like a phantom before the severe truth of philosophical investigations.

M. Fourier discovered that the *excess of the total temperature* of the earth's surface above that which would result from the solar action only, has a necessary and definite relation to the increasing temperature at different depths; and he has deduced the numerical value of this excess from that of the increase last alluded to, as ascertained by experiment; or, in short, he has determined the thermometrical effect at the surface produced by the central heat; but instead of the large numbers assigned to it by Mairan, Buffon, and Bailly, the learned secretary of the *Académie* has determined this quantity to be—the thirtieth of a degree!

Hence it appears, the surface of the globe, which, probably, was originally incandescent, has cooled so much in the lapse of ages, as scarcely to preserve a sensible trace of its primæval temperature; while, at a certain depth below that surface, the heat is still enormous. It is certain that further modifications must be yet brought about in the internal temperature, but all the changes in this respect, of which the surface is susceptible, have been effected to within nearly $\frac{1}{30}$ of a degree, and it is the phenomena which take place at the surface only, which can compromise the existence of living beings. The terrific congelation of the globe, therefore, the epoch of which was fixed by Buffon at the time when the internal heat should be entirely dissipated, is a pure fiction.

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* It may not be irrelevant to mention in what manner so astonishing an exactness has been attained. Suppose, in order to ascertain the constancy of the sidereal day, we were to take the distance passed through by the moon in one day at each epoch, for the basis of our calculations, such as it would be furnished by one direct observation. When employing the best instruments of which modern astronomers are possessed, the arc passed over by the moon in a sidereal day may be measured nearly to *a second of a degree*, and to pass through this arc, the moon takes *two seconds of sidereal time*. If, then, we were to commit an error in excess, of a single second, in determining the lunar motion, it would be equivalent to making the sidereal day longer by *two seconds of time*. This is far from the exactness recorded in the text, and, accordingly, it is not from the observations of one day that the lunar motions are deduced.

Suppose we measured the arc passed through by the moon in ten days, this arc will be ten times as great as the former, but will still be liable to an error of only one second, for this error depends solely on the operations to be performed either at the commencement or at the termination of the observations; and these operations, it is obvious, must be the same whatever may be the length of the arc; when, therefore, in order to obtain the daily arc, we divide that just alluded to by ten, the total error will be diminished in the ratio of 10 : 1, which is equivalent to an original error of $\frac{1}{10}$ of a second of a degree, or to $\frac{2}{10}$ of a second of time.

If we were to measure the arc described in 200 days, and were thence to deduce the mean daily arc by a similar mode of proceeding, the second of error would be reduced to $\frac{1}{200}$ of a second, or to $\frac{1}{100}$ of a second of time, and so on. This will serve to illustrate the assertion in the text.

Is the temperature of space variable, and can this temperature become the cause of changes in terrestrial climates?

M. Fourier has recently brought to light a circumstance in the *Theory of Climates*, which has been entirely overlooked, or, at least, of which *physiciens* have hitherto made no explicit mention; he has determined the influence which the temperature of the celestial space, in which the planetary motions are carried on, and in which the earth annually describes its immense orbit round the sun, must exercise on our climates. When we behold mountains covered with eternal snow, even at the equator, and reflect on the rapid decrease in temperature of the successive strata of the atmosphere, experienced by aëronauts on their ascent through them, it was natural to conclude, that in those regions of the atmosphere where the extreme tenuity of the air for ever precludes the existence of man, and, *à fortiori*, beyond its limits, an intense cold must always prevail, which was *estimated* not by hundreds, but by thousands of degrees. All this has been greatly exaggerated. M. Fourier, by the strictest inductions, has reduced these hundreds and thousands to 50 or 60 degrees only below zero, which then it appears is the temperature of that boundless space the earth circulates in during her annual revolutions: and a thermometer would only indicate that degree throughout the region occupied by our system, if the sun, together with its attendant planets, were annihilated.

This temperature of celestial space, whatever may be its degree, is probably due to the radiation from all the bodies in the universe the light of which reaches us. Many of these have disappeared, others, on the contrary, augment in splendour, but these are both rare exceptions; and since the total number of stars and nebulæ visible with telescopes certainly exceeds many millions, we may conclude that the inhabitants of our globe have no alteration in climate to dread from this source at least.

Do the variations undergone by certain astronomical elements sensibly influence terrestrial climates?

There is but one region of the globe in which, if we set aside the effects of atmospheric refraction, the days and nights are always of the same length; everywhere else than at the equator, the days and nights are unequal at all seasons, save at the equinoxes, when their equality is general over the whole globe. Now it is not necessary to have reflected very profoundly on the subject to perceive, that, generally, in all latitudes, the season of long days and short nights must be that of high temperature and the reverse; in short, that the thermometrical extremes must everywhere have a necessary and intimate connexion with the difference between the longest and shortest days. Every cause which reduces this difference will render the winters and summers more similar. It is not, however, equally evident that a change would hence result in the *mean* temperature, and yet a tendency to any equalization in the seasons would be too remarkable, and have everywhere too decided an influence on the phenomena of vegetation, not to make it worth inquiry whether any such tendency has manifested itself, since the historic era, in consequence of some change in the form and position of the earth's orbit.

During its apparent annual course, the sun is for six months to the north, and for six to the south of the equator; it cannot be denied that at every period of the year the length of the *day* is precisely equal to the time which elapses between the rising and setting of that constellation in which the sun is then situated, and of which it seems to form a part. The question, then, whether the days of winter, compared with those of summer, are more or less unequal now than they were two thousand years ago, resolves itself into, whether the sun has always stopped at the same stars in its excursions to the north and south of the equator? Mathematically speaking, it has not; the sun's declination to the north and south has been gradually diminishing from the earliest ages in which observations have been made*; nevertheless, the annual diminution is excessively minute, since in two thousand years it has hardly amounted to a quarter of a degree: in other words, the sun, at the present period, at the end of its excursion to the south, for example, begins to return again towards the equator when the lower edge of its disk has arrived at that star which its centre attained two thousand years ago. It is obvious so insignificant a variation cannot have effected any perceptible change in the comparative length of the days of winter and summer, nor, consequently, in the phenomena of vegetation.

The sun is not at all seasons equally distant from us; at the present epoch, it is at its least distance during the first days of January, and at its greatest in the beginning of July. The time will come, when, on the contrary, the sun will be at its *minimum* distance in July, and at its *maximum* in January; this circumstance naturally suggests the following interesting question:—Will a summer such as that of the present epoch, which corresponds to the maximum of the solar distance, differ sensibly from one during which this distance would be at a minimum?

At first, all the world, I believe, would reply in the affirmative, for there is a considerable difference, amounting, in round numbers, to $\frac{1}{30}$ of the whole, between the greatest and least distances of the earth from the sun; but when we take into consideration the *velocities*, which must not be neglected, the result will be the reverse of what might at first be imagined.

The portion of the orbit in which the earth is situated when nearest the sun, is also that which it passes through most rapidly. The semi-orbit, or the 180° comprised between the vernal and autumnal equinoxes, will then be passed through in the minimum of time, when the earth, in traversing from one end to the other of this arc, shall pass, towards the middle of its course, through the point of least distance. By the hypothesis above suggested, we ought to have a spring and summer of a higher temperature than they are at present, in consequence of the diminished distance, but on account of the increased velocity, the duration of these two seasons, taken together, would be shortened by about seven days; and it is found, by calculation, that the compensation is mathematically exact. It is almost superfluous to add, that the part of the earth's orbit corresponding to the least distance, shifts its position extremely slowly; and that from the earliest ages the planet has always passed through that point, either at the end of autumn or at the beginning of winter.

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* The variation in the annual oscillations of the sun on each side of the equator is *periodical;* after diminishing, for a certain number of centuries, these oscillations will increase again, and so alternately, without ever exceeding certain limits.

We have just shown that no modifications in terrestrial climates are produced by changes in the *position* of the earth's orbit; let us now examine whether any can be shown to result from variations in its *form.*

The earth's orbit is an ellipse differing but slightly from a circle; the major axis of this ellipse is constantly of the same length; the eccentricity, on the contrary, varies. The constancy in the length of the major axis of a planet's orbit necessitates an equal constancy in the time of the periodic revolution of that planet round the sun; thus, whatever may be the changes which take place in the terrestrial eccentricity, the length of the year will always remain precisely the same.

This being premised, the problem before us may be thus stated:—Does the earth, altogether, receive the same quantity of heat from the sun, whether it revolves during 365·256 days, in a perfectly circular orbit; or in an elliptical one, of greater or less eccentricity, but nevertheless, always having its major axis equal to the diameter of the circle? We can anticipate an answer in the negative to this question, that is to say, that the total quantity of heat received by the earth will augment with the eccentricity of the ellipse, if in imagination we carry this eccentricity to an extreme; if we fancy the orbit so compressed that its two sides shall nearly graze the surface of the sun, and thus compel the earth to touch it twice a-year. Analysis, however, gives the value of the augmentation for every case; it shows us that the earth must receive annually from the sun total quantities of heat inversely proportional to the smaller axes of elliptic orbits, having the major axis invariable.

The eccentricity of the terrestrial orbit is gradually diminishing, or the lesser axis is increasing; the heat, therefore, which we receive from the sun is also diminishing; but in truth this variation in the eccentricity takes place so slowly, that it would require more than ten thousand years to produce a change in the terrestrial temperature appreciable by the thermometer; in regard to the time elapsed since the *historical era,* the influence of this cause may be entirely neglected.

Sir J. Herschel, who has recently paid attention to this problem, in the expectation of deriving from it an explanation of various geological phenomena, admits that the lapse of ages may cause an eccentricity in the terrestrial orbit analogous to that of the planet Pallas, that is to say of being one-fourth of the semi-major axis. It is hardly probable that in its *periodic* variations the eccentricity of our orbit should ever reach such a proportion, yet, nevertheless, even this one-fourth will not augment the *mean* annual solar radiation more than $\frac{1}{106}$; or, an eccentricity of one-fourth would not perceptibly affect the *mean* thermometric state of the globe. The only result would be that, at intervals of six months, the greatest and least distances of the earth from the sun, which at present hardly differ by $\frac{1}{30}$, would in that case be in the ratio of 5:3, and at these proportional distances the illuminating and heating powers of the sun would be as 1:3. Let us suppose this triple intensity to coincide with the summer solstice; that is, let us imagine three suns above us in the months of July and August, and we may form an accurate idea of the excessive heat we should experience *during certain days* if the eccentricity of our orbit was one-fourth. But I must again repeat that such an eccentricity probably never existed, and that certainly it could only have existed from fifteen to twenty thousand years ago*.

[To be continued.]

A POPULAR COURSE OF GEOLOGY.

VI.

EXTERNAL CHARACTERS OF MINERALS.

HAVING described the most remarkable properties of the elementary bodies in their relation to the crust of the earth,—having pointed out the distinction between the simple bodies of chemistry and simple minerals,—and having taken a cursory glance at some of the characters by which minerals are discriminated, we will now resume this latter part of the subject more in detail, and will describe those minerals which enter most extensively into the composition of rocks. It must be premised, however, that the student desirous of obtaining a knowledge of mineralogy will learn more in a few weeks from oral instruction, and from the careful observation of well-arranged collections, than he can in months, or even years, from the most elaborate descriptions, unaccompanied by examination of the substances described.

Facilities for the study of mineralogy have of late greatly increased. Public collections of minerals were, a few years ago, confined to the metropolis, the capitals of Scotland and Ireland, the universities, and one or two of the chief provincial towns. To those, therefore, who resided at a distance from such public collections, or from teachers of mineralogy, that science was in a great measure a sealed book. Their only resource was to purchase from dealers named collections of minerals, the expense of which amounted to a prohibition with the majority of students, or to pick up information, slowly and laboriously, by examining such specimens as chance might throw in their way, and by comparing them with the descriptions in works on mineralogy. But now the case is greatly altered. There are now few counties in Great Britain in which Natural History Societies have not been established, and some of the larger and more opulent counties support two or three. Minerals must ever form a prominent object in Museums of Natural History; and a useful and instructive collection of these, although too costly for most individual students, may be purchased for a sum which becomes trifling when divided among a number of contributors. Many and disheartening were the difficulties which beset us in the commencement of our mineralogical studies, from the want of access to instructors, and of opportunities of seeing minerals; and we never look at even the poorest and worst-arranged collection to be found in the cabinets of any local museum, without thinking what a treasure-house of information it would in those days have been to us.

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* The reader must bear in mind that M. Arago is only discussing the *probable and possible* changes in terrestrial temperature within the period man has existed on the globe; Sir J. Herschel, on the contrary, discusses the probable and possible changes which may account for phenomena which took place in countless ages before our epoch; and it is quite possible that the differences alluded to may be adequate, *in some measure*, to account for them; but Sir J. Herschel only suggested the fact of a diminishing eccentricity as worthy of consideration, but never attached any importance to it as satisfactorily accounting for the phenomena of the primæval world: true philosophers, like M. Arago and Sir John Herschel, seldom start *hypotheses*.

Minerals may be discriminated by their *external* and by their *chemical* characters; and chemical analysis is of two kinds,—*qualitative* and *quantitative*. By means of the former, we merely seek to ascertain what ingredients are to be found in any substance; by means of the latter, we determine the quantities in which these different ingredients are combined. As the chemical characters depend on the composition of minerals, these can only be thoroughly known by means of a complete quantitative analysis; but a rough qualitative analysis will frequently afford considerable insight into their composition, and furnish a series of characters, many of which are peculiar to certain minerals, or groups of minerals, and are thus of great assistance for the discovery of their nature when any ambiguity arises in the external characters. This qualitative analysis may be performed upon a small scale, both cheaply and expeditiously, either in the dry or the humid way. In the first process, small fragments of the mineral to be examined are submitted to the action of heat. The heat employed is that of a lamp or candle, urged by a blast through a blowpipe, this blast being sustained, most commonly, by the lungs of the operator, but sometimes by bellows or other apparatus. The blowpipe is a small tube seven or eight inches long, a quarter of an inch in diameter at its widest end, and gradually contracting till it terminates at the other in an orifice of the size of a small wire. By means of this instrument, an intense heat, equal in degree to that of the most powerful wind-furnace, may be produced. Many earthy minerals may be fused by it; and the ores of many metals may be reduced to the metallic state; and blowpipe-operations have this advantage over those conducted in a furnace,—that the whole process takes place immediately under the eye of the operator, who can thus observe all the changes produced by the action of heat upon the mineral under examination, when treated either alone or with the addition of borax, carbonate of soda, or other fluxes. These changes are termed the habitudes of a mineral before the blowpipe, and are of great value as distinctive characters. We can, by these means, distinguish felspar from quartz,—the former being fusible, the latter infusible unless mixed with soda. A sulphuret may be known from an arseniuret by the different odours which they exhale when heated, and the different metals communicate peculiar colours to the glass formed by their fusion with borax.

In conducting a qualitative analysis in the humid way, a small portion of the mineral reduced to powder is submitted to the action of a few drops of acid in a watch-glass, heat being applied from the flame of a lamp or candle, if necessary; and if the mineral can thus be brought into a state of solution, the solution is diluted with water, and poured into several small glass tubes, to each of which a different test or re-agent is added, and by each of these, peculiar changes are produced in the solution, according to the nature of the substances contained in it.

We shall first describe the external characters of minerals; then the mode of ascertaining their chemical characters by the blowpipe and with acids; and lastly, enumerate the most common of the earthy minerals, giving both their external and chemical characters.

THE EXTERNAL CHARACTERS.

THESE are form, structure, fracture, frangibility, hardness, transparency, translucency, lustre, colour, flexibility, elasticity, double refraction, touch, taste, odour, streak or powder, stain, adhesion to the tongue, magnetism, electricity, and specific gravity or weight.

With respect to form, a mineral is either without any particular form, when it is said to be *amorphous;* or it bears some general resemblance to common substances, natural or artificial, in which case it is said to have a *particular form;* or its natural surfaces consist of a certain number of planes of a determinate shape, arranged in a determinate manner, in which case it is said to be *crystallized*, or of a *regular form.*

A mineral which occurs in pieces not exceeding the size of a hazel-nut embedded or incorporated in another mineral, is said to be *disseminated*, whether it be amorphous or crystallized. If amorphous, and in pieces larger than a hazel-nut, it is said to be *massive.* When the size of the pieces is from that of a hazel-nut to that of a pea, it is said to be *coarsely* disseminated; from the size of a pea to that of a grain of mustard-seed, it is *finely* disseminated; and when the pieces are below that size, and still perceptible by the eye, it is *minutely* disseminated. An amorphous mineral that occurs loose or detached is said to be *in grains*, which are distinguished according to their size, as *gross*, *large*, *small*, and *minute.* Gross grains are from the size of a hazel-nut to that of a pea; large, from the size of a pea to that of hemp-seed; small, from the size of hemp-seed to that of mustard-seed; below which size they are called minute. An amorphous mineral that occurs adhering to another mineral in flattened pieces which do not exceed an inch in thickness, is said to be *in plates.*

Particular external forms are those which bear a resemblance to common well-known bodies, both natural and artificial. The following are the names given to different particular forms:—

Dentiform.—When a mineral adheres by the thicker end, is elongated towards the other end with a curve, and terminates in a point, it is said to be dentiform, or tooth-shaped, from the Latin *dens*, a tooth.

Filiform.—A mineral is said to be filiform that occurs in long slender columns, like wires or threads, from the Latin *filum*, a thread.

Capilliform.—When the threads are more slender, like hairs, the mineral is called *capilliform*, from the Latin *capillus*, hair. These columns or threads are, in fact, imperfect crystals. When a mineral occurs in long, slender, well-defined crystals, it is said to be *acicular*, or needle-shaped.

Retiform.—This term is applied to parallel threads or fibres intersecting other parallel threads or fibres at right angles, and producing the appearance of net-work, from the Latin *rete*, a net.

Dendritic.—This is synonymous with arborescent, and is applied to a mineral which from a thicker main stem separates into several which are more slender, and are often furnished with smaller shoots, presenting, on the whole, the appearance of a tree, whence the name, from the Greek δενδρον, a tree. We have dendritic native silver and copper, and dendritic manganese, which is of very common occurrence on the surface of natural joints in rocks.

Ramose.—This is that particular form which consists of crooked branches not rising from a common stem, nor possessing a proportionate thickness; from the Latin *ramus*, a branch.

Coralliform.—A mineral occurring in elongated curved shoots, rounded at the extremities, is said to be coralliform, from its resemblance to coral.

Stalactitiform.—A mineral resembling an icicle in shape is said to be stalactitiform, from the Greek σταλαγμα, a drop, an icicle.

Tubuliform, or *fistuliform*, are terms applied to minerals occurring in round and hollow columns, single or in groups; from the Latin *tubulus* and *fistula*, a pipe.

Botryoidal, resembling a cluster of grapes; from the Greek βοτρυς, a grape. Minerals presenting an aggregation of large sections of small globes are called botryoidal. This particular form occurs in calcedony, manganese, and malachite, or green carbonate of copper. When the globes are larger, and the sections or portions smaller, the term mammillated is used; from the Latin *mamma*, a breast.

Reniform.—A mineral is said to be reniform, when it consists of several globular elevations, each of which is sometimes composed of several that are smaller. The name is derived from the Latin *ren*, a kidney, from the resemblance to kidneys, particularly those of calves. Red and black hæmatite are good examples of this form.

Specular, from the Latin *speculum*, a looking-glass. A mineral not crystallized, and having one smooth and brilliant surface, is called specular. Specular galena, or slickensides, affords an example. Some crystallized minerals are also said to be specular, from the brilliancy of their planes, as specular iron-ore.

Laminated.—A mineral is said to be laminated, when it occurs in thin plates or leaves adhering to another mineral; from the Latin *lamina*, a thin plate.

Pectinated.—This form is produced by tables disposed upon and beside each other, so that they present the appearance of hair parted by the comb; from the Latin *pecten*, a comb. It is of very rare occurrence.

Cellular.—This term is properly applied to minerals which exhibit the appearance of cells formed by the crossing and intersecting of the laminæ of which they are composed. It is, however, frequently used instead of *vesicular*.

Vesicular.—This term belongs to a mineral having small and somewhat round cavities, both externally and internally, as in lava, pumice, and basalt. It is derived from the Latin *vesicula*, a little bladder.

REGULAR FORMS, OR CRYSTALS.

WHEN a body passes from the gaseous, or fluid, to the solid state, the particles of which it is composed sometimes arrange themselves together so as to form a shapeless mass; but they more commonly dispose themselves in a certain order so as to give rise to solids whose surfaces consist of a determinate number of sides, associated in a determinate manner. Such bodies are called crystals, and the process by which they are formed is termed crystallization. This process takes place when a solid has been fused, and is allowed to return slowly to the solid form, or when it has been dissolved in a fluid from which it is slowly deposited by evaporation; and the more slowly this cooling or evaporation takes place, the more regular are the forms of the crystals. As an instance of crystallization from solution add alum to eight or ten ounces of boiling water as long as any is dissolved, and on allowing the liquid to cool, crystals will be deposited; or, dissolve oxalic acid in water, and on exposing it to the air for some days in a shallow vessel, part of the water will be evaporated and crystallization will take place. As an instance of crystallization from a state of fusion, melt a quantity of sulphur in a large crucible, and when it is partially cooled, pierce the solid crust, and, inverting the crucible, let the fluid parts run out. When it is quite cold, regular crystals of sulphur will be found in the interior. The regularity of the crystals in this case arises partly from their having had free space to form in, as well as from the slowness of cooling, for confused crystallization may frequently be seen in the centre of a roll of sulphur. The crystallization occurs in that part because it was the longest in cooling, and it is imperfect, first, because the cooling even of the interior of so small a mass takes place too rapidly for the formation of any but irregular crystals, and secondly, because they have no vacant space in which to arrange themselves without interfering with one another. Sometimes analagous is met with occasionally in granite. That rock is an irregularly crystallized mass of quartz felspar and mica, like the imperfect crystallization in the stick of sulphur, but some kinds of granite, as that of the Mourne Mountains in Ireland, abound in cavities in which those minerals are found in regular crystals. The most perfect crystals of mineral veins are always found lining similar cavities.

Crystallography, or the study of the forms and structure of crystals, is an extremely interesting and important department of science, by which mineralogy is connected with mathematical investigations and the most refined optical researches.

It has been found that certain forms are so far peculiar to certain minerals and salts as to constitute grounds of distinction between them.

Calcareous spar crystallizes in rhombohedrons, fluor spar and in cubes, quartz in six-sided pyramids; but fluor spar is never found in rhombohedrons, nor calcareous spar and quartz in cubes.

The surfaces which bound the figure of a crystal are called planes or faces. They are most commonly flat. The lines formed by the junction of two faces are called edges. The angle formed by the junction of two edges is called a *plane angle*. The point formed by the meeting of at least three planes is termed a *solid angle*. Thus in the cube (fig. 4), *a a a* are planes, *b b b* are edges, and *c c* are solid angles. *Lateral planes* are those of the greatest extent, considered as parts of the surface, and they bound it towards its smallest extent. *Extreme* or *terminal* planes are those of the smallest extent as parts of the surface, and form its confines towards its largest extent. Lateral edges are formed by the junction of lateral planes with lateral planes. Terminal or extreme edges are formed by the junction of lateral planes with terminal edges. Thus, in the right square prism (fig. 5) *a a* are lateral planes, *b b* terminal planes, *c c* lateral edges, *d d* terminal edges. Crystals sometimes present the appearance of having lost their edges and solid angles, which are then said to be *replaced* by tangent planes. A *tangent plane* with reference to an edge means a plane inclined equally to the two adjacent primary planes, and parallel to the edge which it replaces. With reference to a solid angle a tangent plane is inclined equally to all the planes constituting the solid angle. When an edge is replaced by two small converging planes, terminating in an edge, it is said to be *bevelled*. When a solid angle is replaced by three or more converging planes, it is said to be *acuminated*.

Fig. 4.

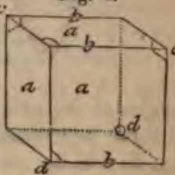
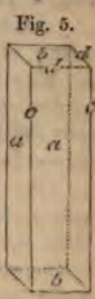


Fig. 5.



The forms of crystals are divided by crystallographers into *primary*, (called also *primitive, fundamental*, or *derivative*, and *secondary*, or *derived*) forms. This distinction is founded on the relation of certain geometrical solids to each other,—on the transitions from one form to another exhibited by many minerals from the replacement or truncation of their edges and solid angles,—and on the facility with which most crystallized minerals split in certain directions, so that however various the forms of their crystals, they may all be reduced by cleavage into some simpler form, having smooth shining planes like the natural faces of a crystal. If the solid angles of a regular octohedron (fig. 6) be cut off, truncated or replaced, as in fig. 7, and if these planes be equally enlarged till the faces *of the octohedron* disappear, a cube will be formed; or if the

Fig. 6.

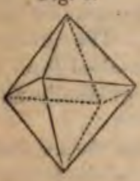


Fig. 7.

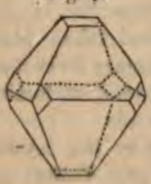


Fig. 8.

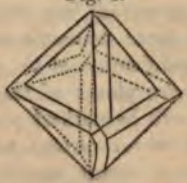
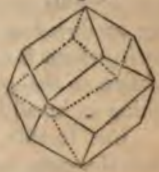


Fig. 9.



twelve edges of the octohedron be replaced by tangent-planes, as in fig. 8, and if these be extended till they mutually intersect, the rhombic dodecahedron, fig. 9, will be produced. In the same manner, if the eight solid

Fig. 10.

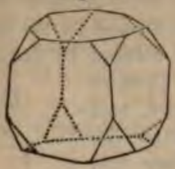


Fig. 11.

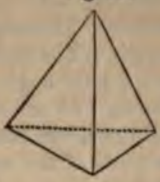
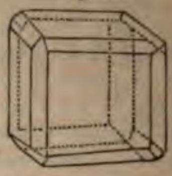


Fig. 12.



angles of the cube be replaced, as in fig. 10, by equilateral triangles and if these be extended till the planes of the cube disappear, the octohedron, fig. 6, will be the result. The tetrahedron, fig. 11, may be formed by the replacement of the four solid angles, *cc*, *dd*, of the cube, fig. 4, till the cubic faces are destroyed, and the rhombic dodecahedron, fig. 9, may be formed by replacing all the edges of the cube, as in fig. 12, with tangent-planes, till they intersect.

In order to render these relations more striking, and to show the variety of secondary forms which may be produced by combinations of the planes of different primary forms, let the student cut cubes and octohedrons out of an apple or a turnip, and practise the replacements of their solid angles and edges described above, till the cube results out of the octohedron, and the octohedron out of the cube.

For an enumeration of the other primary forms, which are differently stated by different authors, and for an account of all the secondary forms derived from them, we must refer to works on crystallography. Not only is there this relation between certain geometrical solids, so that by the truncation of their edges and solid angles one may be derived from the other, but the intermediate forms occur in nature. Cubic and octohedral crystals, for instance, are met with in which the replacement is so slight that the general form of the octohedron or cube remains, (see figs. 7 and 10,) while in others, the replacement is so deep that the primary planes are hardly distinguishable.

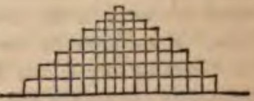
In speaking thus, it is not meant, that the edges and solid angles have actually been removed, but only that the same appearances have been produced as if these truncations had taken place. The probable way in which the modification of the primary form has resulted from the addition to the primary planes of the crystal of laminæ gradually diminishing in breadth, will be noticed presently.

But further, however various may be the crystalline forms under which the same mineral occurs, and however distant may be the localities from which different specimens of it may have been brought, if they yield to cleavage at all, they may all be cleaved so as to afford the same nucleus or primary form. Calcareous spar is found crystallized under three hundred varieties of form at least; but these may all be cleaved in three directions, and in those directions only, so as to give bright surfaces like the faces of a crystal; and those cleavages are so inclined to each other as to produce an obtuse rhomboid, having plane angles of invariable measurements. Into this form, and no other, are all crystals of carbonate of lime cleavable, and therefore this is justly considered as its primary form. The primary forms of some other minerals have been established on more arbitrary grounds. Fluor-spar, may be cleaved in four directions, so as to produce three different forms, the regular octohedron, the regular tetrahedron, and an acute rhomboid: out of these crystallographers have selected the first as the primary, though for what reason is not very apparent. Crystals of blende, or sulphuret of zinc, may be cleaved into five different forms, one of which has been fixed on as the primary. There are other minerals whose crystals admit of cleavage in only one direction; and yet to these, and to others which are not susceptible of cleavage in any direction, primary forms have been assigned. In such cases, primary form has been deduced from the direction of natural fissures or joints which become visible when a very thin slice of the mineral is held up between the eye and the light, or it has been inferred from analogy by comparing the crystalline forms of the substance under investigation with others whose primary forms are well ascertained.

We have spoken of the formation of the cube out of the octohedron, and of the octohedron out of the cube, by the replacement of their solid angles; but either of these bodies may be formed upon the other, by the addition of layers parallel to the planes of the original solid, such layers gradually diminishing in breadth. For example, when cutting off the solid angles of a cube, and extending the planes produced thereby, if we preserve the layers successively removed, and fasten them on again, we shall have the cube formed upon the octohedron by addition, as the octohedron was formed out of the cube by diminution; and in the same manner, after reducing the octohedron to the cube, we may form the octohedron on the cube, by restoring the layers which had been cut off.

When, therefore, we speak of the modification of the primary form of a crystal by the replacement, or truncation, of its edges and solid angles, we merely adopt a fiction which affords a convenient mode of describing effects produced by a process of which we are ignorant, such process having, in all probability, been the very reverse of that indicated by the terms employed. For there are good grounds for considering the secondary forms of crystals as having arisen from the regular arrangement, on the planes of the primary forms, of laminæ of crystalline matter, gradually decreasing in breadth, such laminæ being made up of extremely minute particles, all of the same form, in the same manner that a pyramid may be built of regularly-diminishing horizontal courses of small cubical bricks, each course or layer being a plate of the thickness of a single brick. A pyramid built of such bricks would, of course, present the rough channeled outline seen in fig. 13, instead of the smooth polished faces of a crystal; but if each of these bricks were (as we may conceive the integrant molecules of crystals to be) so small that the inequalities produced by them were not discernible, the faces of the pyramid would appear as smooth as those of a crystal.

Fig. 13.



The form of ultimate particles which are so small as to be invisible, opens, of course, a wide field for conjecture. It is, however, certain, that secondary crystals *may* be cleaved into layers parallel to the planes of a primary nucleus of a different form, (in some large and coarse crystals these layers are visible, being marked by striæ on its surface); these layers may be divided by cleavage in other directions, so as to afford small bodies of a determinate shape; these again may be divided and subdivided into smaller solids, all of the same shape, till they are no longer discernible without the aid of a lens; and they still preserve their peculiar shape as long as they are visible. These forms, then, whether cubes, octohedrons, &c., are considered as the forms of the constituent molecules of which the crystals are built up.

By placing a low three-sided pyramid on each plane of the regular octohedron, a rhombic dodecahedron may be formed, as in fig. 14, in which the strong lines mark the octohedral nucleus, and the fainter lines the planes of the dodecahedron, each of which is a rhomboid composed of two triangular planes, belonging to two different pyramids. On one face of the primitive are also marked the progressively-decreasing layers of which the pyramid is made up.

Fig. 14.

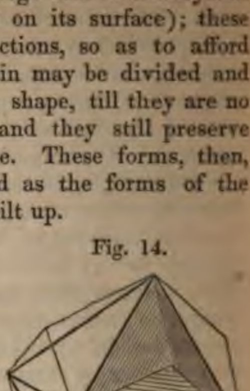


Fig. 15.

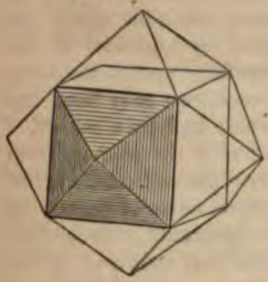


Fig. 16.

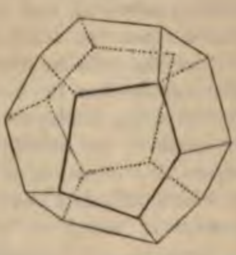
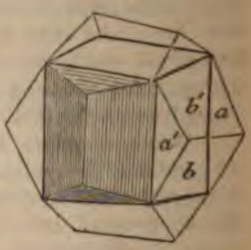
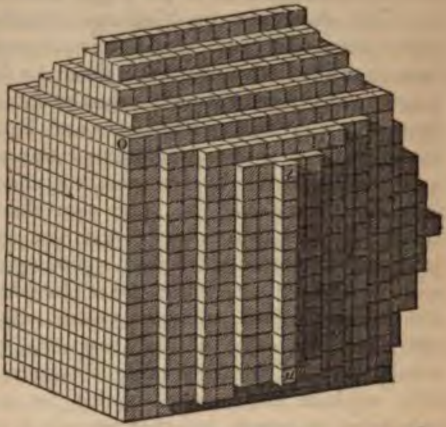


Fig. 17.



The rhombic dodecahedron may also be formed by placing a low quadrangular pyramid on each face of the cube, as in fig. 15; and the pentagonal dodecahedron, fig. 16, may likewise be formed on the cube, by placing on each of its faces an equal and similar pyramid, having two triangular and two quadrangular planes, as in fig. 17. Now, in the formation of these two different dodecahedrons on the cube, if the integrant molecules (*the bricks*) of which the two kinds of pyramid are composed be all of a cubic form, there must be a very different arrangement of them in the two cases; for, in the rhombic dodecahedron, the faces of the pyramid are all equal, and inclined on the faces of the cube at the same angle; but in the pentagonal dodecahedron, the faces of the pyramids are only equal and similar in pairs *a* to *a* and *b* to *b*, and they are inclined on the planes of the cube at different angles. These angles have been measured by instruments called goniometers, and calculations have been made for determining the mode of aggregation of particles of the same shape necessary to produce these different forms; and it has been ascertained, by these means, that the pyramids of the rhombic dodecahedron must be composed of layers or molecules placed in regular order, each layer being of the thickness of one molecule, and each successive layer diminishing regularly by the breadth of one molecule on each side; but that in the case of the pentagonal dodecahedron, the layers of its pyramids must be of the thickness of two molecules, and must diminish in breadth unequally on the two sides; that is to say, on the side of the quadrangular plane they must diminish two molecules in breadth for one of height, and on the side of the triangular plane they must diminish one molecule in breadth for two of height, as shown in fig. 18. The Abbe Haüy investigated in like manner the angles under which the secondary planes would meet, arising from gradual decrement in the dimensions of successive layers of crystalline matter deposited on the other geometrical solids as the primary nucleus. He may be considered as the founder of crystallography. Guided by the accidental fracture of a crystal of calcareous spar, he discovered that all crystals have a primary form, which may be extracted from them by mechanical division. He calculated the modifications of which those primary forms are susceptible, and determined the combinations of integrant molecules necessary to produce different forms of crystal with molecules of the same shape, and to produce like forms with molecules of different shapes; and in all cases it has been found, that the results obtained by measurement of the angles, and by calculation, mutually confirm each other.

Fig. 18.



On these grounds we infer, that regularity of external form arises from regularity of internal structure, dependent on the combination, under definite laws, of minute ultimate particles of determinate shapes.

Crystalline form and structure are, therefore, of too much importance to be passed over wholly unnoticed, in an account of the external characters of minerals, and it has been impossible to give even the above imperfect outline, without entering into considerable detail. At the same time it must be acknowledged, that crystallography must be considered as affording a field for refined and curious research, rather than as furnishing, to any but experienced mineralogists, available practical rules for discriminating minerals. In calcareous spar, and some other substances, the ease with which they may be reduced to the primary form renders it a good distinctive character; and a knowledge of the crystalline forms in which certain minerals are most commonly found, will often assist in identifying a mineral with which we are unacquainted, when it is rendered ambiguous by the possession of several external characters in common with other minerals; but, in general, before the student can be able to obtain much information by cleaving crystals into their primary forms, and measuring their angles, he will have learned, by other means, the names and composition of most, if not all, the minerals which are of much importance to the geologist. The beginner, therefore, whose object is to discover the names of minerals of which he is ignorant, and the working geologist, who most commonly meets with them massive, or only irregularly crystallized, must have recourse to a combination of characters more obvious, though individually less definite, than the measurement of the angles of crystals.

The discovery, by Professor Mitscherlich, of what is called the *isomorphism* of crystals, diminishes in some degree the value of crystalline form as a distinctive character, since he has found that certain bodies may be substituted for each other in combination, without affecting the form of the crystals. Phosphate and biphosphate of soda crystallize in the same forms as the arseniate and binarseniate of the same alkali; and each arseniate has a corresponding phosphate, possessed of the same form, and containing the same equivalents of water, acid, and alkali. Hence, those bodies which crystallize in the same forms have been arranged in groups, called isomorphous groups. The salts of arsenious acid are isomorphous with those of phosphoric acid; oxide of lead, baryta, and strontia, afford, in combination with the same acid, isomorphous salts; and the salts of lime, magnesia, protoxide of manganese, iron, cobalt, oxide of zinc, and oxide of copper, are all isomorphous. We will now explain the terms applied by mineralogists to crystals when aggregated in groups, and then proceed to consider the remaining external characters.

Crystals are said to be *fasciculated*, when they occur aggregated in small bundles, from *fascis*, Latin, a bundle.

Scopiform, is a term applied to a number of minute crystals or fibres closely aggregated into a bundle, and diverging slightly from a common centre, from the Latin *scopa*, a broom.

Stellated. Minerals are said to be stellated, when their crystals or fibres diverge all round a common centre, from the Latin *stella*, a star.

Drusy. Minerals are said to be drusy, when their surface is composed of very small and prominent crystals, nearly equal to each other in size. The name is derived from the German.

Investing. This term is applied to a mineral which coats or covers another.

FRACTURE.—We have shown that most minerals split more easily in certain directions than in others, and that they may thus be reduced in the direction of natural joints into figures, bounded by planes of a smooth shining surface, and a determinate shape. This is their cleavage. The surface produced by breaking them in directions contrary to these natural joints, is called the fracture; and its appearance, which is dependent on the composition of a mineral, forms in most cases a good distinctive character.

The surface of a fracture is said to be *compact*, when no particular or distinct parts are discernible. It is the most common kind of fracture, and is divided into *splintery*, *even*, *conchoidal*, *uneven*, *earthy*, and *hackly*.

The surface is *splintery*, when destitute of any remarkable elevations, but small splinters, or scales, are discernible, thickest at the end by which they adhere, and terminating at the other in an edge. This kind of fracture occurs in hornstone, many varieties of quartz, and some sorts of limestone.

Even is that kind of fractured surface which is nearly flat, and destitute of elevations, such as occur being indeterminate in shape, and flat. It passes into splintery and conchoidal.

Conchoidal surfaces consist of round elevations and depressions, accompanied sometimes by circular wrinkles, having something of the appearance of a shell. It is divided into the *perfect* and *imperfect*, *flat* and *deep*, *large* and *small*, conchoidal.

The best examples of this kind of fracture occur in the most pellucid kinds of quartz, in opal, flint, obsidian, and cannel-coal.

Uneven.—The surface of this kind of fracture is completely broken, and interrupted by irregular and rather large angular elevations and depressions. It is met with in the gray and yellow sulphurets of copper, and many other minerals.

Earthy.—The surface of the earthy fracture is composed entirely of small rough elevations. It is most common in indurated clay, chalk, and the earthy minerals.

Hackly.—In this kind of fracture sharp-pointed parts protrude from the internal surface. It is peculiar to the malleable metals. The fracture of metallic copper is a good example.

Sometimes the fracture presents the appearance of an aggregation of fibres, which are frequently the result of imperfect crystallizations, and are either *coarse*, *large*, *small*, *fine*, or *delicate*, *curved*, *straight*, *parallel*, *diverging*, or *promiscuous*.

The diverging fibrous fracture is divided into *stellated* and *scopiform*, the meaning of which has been already explained.

The fibres are *promiscuous*, when they intersect each other in various directions. We have examples of this structure in alabaster and red hæmatite; of the stelliform fibrous, in some kinds of hæmatite and fibrous zeolite; of the scopiform, in fibrous malachite; and of the promiscuous, in gray antimony.

The fracture is said to be *striated*, when it is marked by narrow grooves or channels. Examples occur in some kinds of sulphuret of iron, in actynolite, and hornblende. The striated, like the fibrous structure, is parallel, stellated, scopiform, and promiscuous.

Foliated or *lamellar* is a term applied to the fracture of those minerals that are composed of thin leaves or plates laid upon one another. Mica, talc, and micaceous iron, are instances of it.

Slaty.—Uncrystallized minerals that split only in one direction, affording fragments that have parallel sides, but are of unequal thickness, not smooth, and destitute of lustre, are said to possess a slaty fracture.

Hardness.—Minerals are found under every degree of hardness, from that of the diamond, the hardest of known substances, down to the fluid state of mercury and naphtha. The different degrees of hardness in minerals are estimated by their scratching or not scratching one another or glass, and by their yielding or not yielding to the file, the knife, and the nail. Calcareous spar will scratch selenite (or crystallized gypsum); fluor-spar will scratch calcareous spar; felspar will scratch fluor-spar, calcareous spar, and selenite; and fluor-spar will not scratch glass, which receives an impression from quartz and felspar. Several of the gems again scratch quartz, and among them different degrees of hardness exist, determined by their making or not making an impression on one another. The giving of sparks with steel is sometimes considered a test of hardness; but it is by no means a good criterion, for flint gives more numerous and brilliant sparks than many substances which are much harder. Minerals are said to be *extremely hard*, on which the file makes no impression; *very hard*, if they receive a weak impression from the file,—such are quartz, flint, and topaz; *hard*, if they receive a considerable impression from the file, but cannot be scraped by the knife,—such as felspar and schorl.

Half-hard minerals yield to the knife, as calcareous spar, limestone, fluor-spar, and many others. By yielding to the knife is not meant a capability of being divided along natural joints, so as to produce smooth and shining surfaces; but the receiving of an impression when scratched or scraped in directions contrary to those joints.

Soft minerals may be easily scraped with the knife, but receive no impression from the nail. Such are, sulphate of baryta, mica, asbestos, and amber.

Very soft minerals are those which yield to the nail, as gypsum, chalk, and chlorite.

Sectile minerals are those which may be cut into slices with a knife, the new surface thus produced being smooth and shining. Examples occur in plumbago and soapstone.

FRANGIBILITY.—This is a property distinct from hardness, for flint, which is much harder than hornblende, is much more brittle. Different degrees of frangibility may be observed, from hard to tough and malleable. The integrant particles of brittle minerals are, in the highest degree, coherent or immoveable among themselves; they are distinguished by the ease with which they are converted into fragments by a blow, and by the fine dust which arises from them on the powerful application of a knife. All hard, many half-hard, and some soft, minerals are brittle. Tough materials are bruised and suffer depression from a blow, and are with difficulty reduced to fragments. Hornblende, basalt, and, in general, all minerals containing hornblende or augite, are remarkable for their toughness. Malleability is the property of suffering extension when beaten with a hammer, and is confined to those minerals possessing a metallic lustre.

TRANSPARENCY.—This character varies considerably in the same substance, and even in different parts of the same specimen. A mineral is said to be transparent, when objects may be distinctly seen through it; *semi-transparent*, when they cannot be seen through large, but only through small pieces, and then but indistinctly. The difference between transparent and translucent minerals is best seen by laying them on printed paper, and observing the distinctness of the letters seen through them. When an object is not visible through a mineral, and in the mass *it is quite impervious to* light, we shall yet sometimes find, on holding it between the light and the eye, that it is *translucent at the edges.* When this is not the case, it is described as *opaque.*

LUSTRE.—Lustre is the relation which a fossil bears to the reflection of light, occasioned partly by the smoothness of the surface or of the aggregated particles of which the surface consists, and partly by its density. Many minerals possess both external and internal lustre, of which the last is the most constant and the most important as a characteristic.

With respect to intensity of lustre, the following different degrees may be established.

The *resplendent* lustre.—A mineral which exhibits, even at a distance, a dazzling lustre, is said to be resplendent, as native mercury, and the external surface and fracture of rock-crystal and sulphuret of lead.

Shining.—A mineral is said to be shining, when its lustre, though perceptible at a distance, is not so distinctly observed as on a nearer approach; such are sulphate of baryta, most calcareous spar, and pitchstone.

Weakly-shining.—This degree of lustre is perceptible only at a short distance, and even then appears rather feeble; it is seen in gray copper, sulphuret of iron, most kinds of quartz, and garnet.

Glimmering.—A mineral is said to have this kind of lustre, when only some of the minute aggregated parts of a fossil reflect light; such are glimmering earthy talc, compact sulphuret of lead, and hæmatites.

Dull.—A mineral, whose surface is wholly destitute of lustre, is termed dull. Hornstone, chalk, marl, and most earthy minerals, may serve as examples.

The different kinds of lustre, arising from the different degrees of density, are the—

Vitreous, or *glassy* lustre, resembling that of the fracture of glass. This is seen in common quartz, rock-crystal, calcareous spar, and fluor-spar.

The *waxy* or *greasy* lustre resembles that of wax. Chloride of silver (or horn-silver) and opal are examples of it.

The *resinous* lustre resembles that of the fracture of gums or resins; jet, amber, and cannel-coal, exhibit this kind of lustre.

The *pearly* lustre resembles that of mother-of-pearl; it exists but rarely except in minerals that have a lamellar structure. Zeolite and adulacia afford examples of it.

The *silky* lustre is peculiar to minerals having a fibrous structure, as in satin-spar and malachite. It is often accompanied by a changeable play of light called by the French *chatoyant;* of which the cat's-eye and Labrador felspar furnish such beautiful examples.

The *adamantine* lustre is that of the diamond and corundum.

The *metallic* lustre is seen in the metals in their metallic state, as in native mercury, native gold, and on the polished surfaces of copper, silver, and steel.

The *semi-metallic* lustre is intermediate between the metallic and the vitreous; mica, and sometimes common coal, exhibit it.

The *pseudo-metallic* lustre is a species of metallic lustre peculiar to a few minerals, and perceptible only when held in particular directions, as in hypersthene and bronzite.

COLOUR.—Colour is not of much importance as a distinctive character, since there are very few earthly minerals in which it is at all characteristic, the same mineral frequently occurring under every variety of shade, of a great many different colours. The earths, in their pure state, are nearly white; and the varieties of colour met with in their combinations arise from the presence of small portions of different metallic oxides as accidental ingredients. In some of the metalliferous ores, however, the internal colour is less variable, and sometimes becomes a good characteristic.

FLEXIBILITY AND ELASTICITY.—These are properties confined to a few minerals, of which they are therefore highly characteristic. A mineral is said to be *flexible*, which, on being bent, does not recover its former shape. If it returns to its original shape after being bent, it is said to be *elastic*. Mica and talc are thus distinguished from each other, mica being elastic, talc merely flexible.

DOUBLE REFRACTION.—Several crystallized minerals, but more particularly a very pellucid variety of calcareous spar, possess the property of doubling the image of objects seen through them in certain directions. This arises from the ray of light by which the object is seen, being split in its passage through the crystal into two, which make a certain angle with each other, and this effect is probably produced by the form of the component particles of the crystal. Double refraction may be considered rather as a curious property of those minerals that possess it, than as a distinctive character. So far from being characteristic of calcareous spar, in which it is found in the greatest perfection, it is confined to one variety of that mineral, called Iceland spar, and sometimes doubly-refracting spar.

TOUCH.—This is a good distinctive character in a few minerals. Some, when handled, feel smooth or unctuous, others dry and meagre. Fullers' earth, steatite, talc, and plumbago, may be cited as examples of the unctuous, chalk and gypsum of the meagre feel.

TASTE.—This is confined to the saline minerals, which are soluble in water, many of which are distinguishable by their peculiar tastes.

ODOUR.—This is by no means a general character, being found in not more than about twelve kinds of minerals; but of those it is very essential as a characteristic. There is the bituminous odour, in mineral pitch and naphtha, and the sulphureous in native sulphur and sulphuret of antimony, which they give out of themselves. Others exhale peculiar odours on being breathed on or rubbed. We have already spoken of the *argillaceous* odour given out by minerals containing alumina, when breathed on, and of the *fetid* odour of swinestone when rubbed, arising from the presence of sulphuretted hydrogen; other minerals, when rubbed, give out the *sulphureous* odour as iron pyrites, and the *alliaceous*, or odour of garlic, as arsenical pyrites and white arsenical cobalt, or the *empyreumatic* odour, as coal and quartz.

STREAK, OR POWDER.—When a mineral is scratched with a knife, it yields a powder sometimes of the same colour as itself, sometimes different. This is called the colour of the streak, and is in many cases a good distinctive character, particularly of some of the metallic minerals, which in other respects possess considerable resemblance to one another.

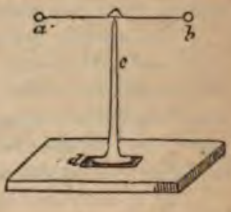
Specular iron, and miraceous iron give a streak of a crimson red; black slate a gray, and hornblende a greenish-gray streak.

Stain.—By the stain is meant the colour left upon paper by drawing a mineral along its surface. Thus the stain left upon paper by plumbago is lead-coloured, that of red ochre and red scaly iron ore red.

Adhesion to the Tongue.—This property arises from the affinity of the mineral for moisture, and is very characteristic of the few that possess it. Lithomarge and chalk are examples.

Magnetism.—This character is confined to the metals iron, nickel, and cobalt, particularly the first, the ores of which differ considerably in their powers of attraction, some attracting iron-filings, others only affecting a magnetized needle; some attract both its poles, others attract one pole and repel the other, and are then said to be magnetic with polarity. Most varieties of sulphuret of iron become magnetic after having been roasted to drive off the sulphur, but one kind, thence called magnetic pyrites, affects the needle in its natural state.

Electricity.—This, like double refraction, must be considered rather as a curious property of the minerals in which it is found, than as a distinctive character of much importance. Some minerals, more particularly the tourmaline, acquire electric properties on being heated, the existence of which is ascertained by means of an instrument called an electrometer. This consists of a light brass needle, *a b*, fig. 19, having a brass ball at either end, and traversing easily on a pivot in the centre of the support *c* (which may also serve as a support to the magnetic needle); *d* is a plate of glass, by means of which the instrument is insulated, glass being a non-conductor of electricity.



There are two kinds of electricity, positive and negative. The first is produced by exciting smooth glass by friction, the latter by exciting sealing-wax or resins. When two substances possess the same kind of electricity, whether positive or negative, they repel each other; but when they are in different states of electricity, they attract each other. In order to ascertain the kind of electricity possessed by a mineral, we must first bring the electrometer into a known state of electricity. This may be done in the following manner: a stick of glass being excited, we place a finger on *c*, the metallic base of the instrument, and then bring the excited glass near one of the balls of the needle, holding it there for a few seconds until the instrument is sufficiently electrified. The finger is first to be withdrawn, and then the glass stick. The instrument will now have acquired negative electricity, and should the excited body presented to it be positively electrified, it will attract the needle, and repel it if negatively electrified. On heating a crystal of tourmaline, it will be found to have acquired positive electricity at one end, and negative at the other.

Phosphorescence.—This is likewise a character confined to a very few minerals, nor does it seem essential to those that possess it. It is best exhibited in fluor-spar; but there are some varieties of that mineral which are not phosphorescent. Phosphorescence may be produced either by heat, as in fluor-spar, or by friction, as in some varieties of blende, and in quartz. The best mode of exhibiting phosphorescence by heat is by reducing the mineral to powder, and sprinkling the powder on a shovel made nearly red-hot.

SPECIFIC GRAVITY.—This is a very important character which is of itself sufficient to distinguish different substances bearing in other respects a close resemblance to each other. Equal bulks of different bodies differ in weight; they are of different densities, or contain different quantities of ponderable matter in the same space; and by specific gravity is meant the weight of a given bulk of any body, compared with an equal bulk of another body. If a cubic foot of water weigh 1000 ounces, a cubic foot of iron 7000 ounces, and a cubic foot of stone 3500 ounces, the iron will be just twice the specific gravity of the stone, and the specific gravities of these two bodies are to that of water in the ratio of 7 to 1 and $3\frac{1}{2}$ (or 3·5) to 1.

The weight of distilled water (a cubic foot of which, at the temperature of 40° Fahrenheit, weighs 1000 ounces,) has, for convenience, been assumed as the standard of comparison; and by comparing the weight of given bulks of different bodies with the weight of an equal bulk of water, considered as unity, we obtain a series of numbers representing their comparative density, or specific gravity.

The specific gravity of such bodies as are lighter than water, are expressed in decimals, and that of bodies heavier than water by integers and decimals; thus, in the example above given, a block of stone weighing 3500 ounces to the cubic foot, would be $3\frac{1}{2}$ times heavier than water, and its specific gravity would be expressed by the decimal 3,500, or its equivalent decimal, 3·5.

If a body be weighed in air, in the common manner, and then being suspended by fine thread or hair to one end of a scale-beam, be weighed immersed in water, it will appear to have lost part of its weight. This loss arises from the support which the body receives from the upward pressure of the water (fluids pressing equally in all directions), and it is equal to the weight of a quantity of water of the same bulk as the body immersed. The weight of a body in air is called its absolute weight, and its loss, when weighed in water, gives the absolute weight of an equal bulk of water. Here, then, are the absolute weights of two bodies of equal bulks; and the proportions which these bear to one another, taking the weight of water as unity, is the proportion of their specific gravities.

To find, therefore, the specific gravity of a body heavier than water, weigh it both in water and out of water. Then say,

As the loss of weight in water
Is to the absolute weight,
So is the specific gravity of water
To the specific gravity of the body.

Thus, if a piece of stone weigh 10 lbs. in air, but in water only $6\frac{3}{4}$ lbs., then $10 - 6·75 = 3·25$, the weight lost in water, and 3·25 : 10 :: 1 : 3·077, the specific gravity of the stone.

To find the specific gravity of a body lighter than water, annex to it a piece of some other body heavier than water, so that the mass compounded of the two may sink: weigh the denser body and the compound mass separately, both in water and out of it. Then find how much each loses in water, by subtracting its weight in water from its weight in air, and subtract the less of these remainders from the greater. Then say,

As the last remainder
Is to the weight of the light body in air,
So is the specific gravity of water
To the specific gravity of the body.

If, for example, a piece of wood weigh 15 lbs. in air, and a piece of copper affixed to it to make it sink weigh 18 lbs. in air, and 16 lbs. in water, the compound mass weighing 6 lbs. in water, then

$18 - 16 = 2$ weight lost by the copper in water,
$15 \times 18 - 6 = 27$ weight lost by the compound mass in water.
$27 - 2 = 25$,
and $25 : 15 :: 1 : 6$, the specific gravity of the wood.

To find the specific gravity of any sort of fluid, weigh a piece of any substance of known specific gravity, both in the fluid and out of it, and find the loss of weight by taking the difference of the two. Then say,

As the absolute weight
Is to the loss of weight,
So is the specific gravity of the solid
To the specific gravity of the fluid.

A piece of cast-iron, for example, weighed 34·61 ounces in a fluid, and 40 ounces of it. What was the specific gravity of the fluid?

$40 - 34{\cdot}61 = 5{\cdot}39$ weight lost by the iron in the fluid,
Then $5{\cdot}39 : 40 :: 1 : 7{\cdot}422$ specific gravity of the iron, and $40 : 5{\cdot}39 :: 7{\cdot}422 : 1$, the specific gravity of the fluid.

The specific gravity of solids and fluids may also be taken by means of a small bottle, called a specific-gravity-bottle, of known weight, and capable of containing a known weight of water. With this the specific gravity of a fluid may be found merely by filling the bottle, and weighing it; that of a solid may be found by filling the botle with water, and then putting in a known weight of the solid in fragments, which will of course displace an equal bulk of water, the weight of which is to be ascertained by weighing the bottle after having wiped its outer surface quite dry.

Different specimens of the same kind of mineral vary a little in specific gravity from the accidental mixture of foreign ingredients, or from their component parts being more or less intimately combined. Crystals, which are the most pure form of a mineral, exhibit the least difference in this respect, and those that are massive the greatest, particularly the ores of metals; but a sort of standard may be established for each kind of mineral by taking the mean of a number of specimens of as great purity as can be procured, and this mean specific gravity is of considerable value as a distinctive character.

ACCOUNT OF SOME NEW EXPERIMENTS IN ELECTRO-MAGNETISM.

Munich, 23rd December, 1836.

At a recent meeting of the Academy of Sciences of this place, Prof. Steinheil communicated to the Section of Mathematics and Natural Philosophy, an extract from a letter he had received from Prof. Gauss of Göttingen, of which the following is a translation:

"Several new electro-magnetic experiments have been made. Induction-currents, transmitted through a person who, with moistened hands, formed part of the chain, were observed to be unaccompanied by any perceptible sensation, though they were indicated by the galvanometer.

"It was at once perceived that the principal resistance to the current lay in the epidermis; for the galvanometer proved, that its intensity was nearly as great when made to traverse the whole body from one hand to the other, as when it passed in at one point, and out at another, of a single hand. Hence the inference, that the current would gain greatly in sensibility, if its ingress and egress took place at parts of the body where the skin is comparatively thin. This was fully borne out by experiment, and a very perceptible sensation was now observed.

"When the currents were applied to the tongue, gums, &c., sharp flashes were also visible; and on directing the stream through places on the arm whence the skin had been removed, the sensation became extremely painful.

"The most important observation, however, appears to be the following. On the current being applied to both lips, its *direction* may be ascertained with the greatest possible accuracy; this experiment, repeated a great number of times, never failed. The current is uniformly perceived upon that lip through which the negative stream is entering (something like a keen wind) and indeed on that lip *alone* when it is but of moderate intensity; and it completely predominates there, even when the current is strong.

"It is also very remarkable that though the effect, as estimated by the amount of the needle's deflection, scarcely varies at all, be the coil slowly or quickly moved (a fact in accordance with theory, and already more than once alluded to publicly by me), yet, the intensity of the sensations produced, depends materially on the rapidity of its movement, so that shocks which at first are barely perceptible, can at pleasure be increased in strength until they can be no longer endured.

"Additional information is thus gained respecting the nature of common electricity; with regard to the electro-*magnetic* effects of which I have instituted numerous experiments. (Colladon was, as is well known, the first to obtain certain results, which on their announcement were much questioned; Faraday, however, subsequently confirmed them.)

"I have succeeded very satisfactorily in the experiments, and common electricity is now, *when due precaution is taken*, transmitted, without any considerable, or even sensible, loss, along our whole chain, from the Cabinet of Natural Philosophy up to the Observatory; and this with as much certainty as if we were experimenting with a hydro-magnetic or an induction-current.

"A few weeks ago I had my inductor increased again, (from 3527 to about 6800 convolutions,) and now its effects are much stronger marked than they formerly were. The sensations it produces by the current being transmitted through the body (from hand to hand, and without, as we used to do, scarifying the skin, but merely by wetting it, and in the case of Dr. Goldschmidt even without that,) are not only very perceptible, but, when the inductor is rapidly moved, painful almost beyond endurance.

"I have been most struck by my success in getting *sparks* very lately, by *fixing fast* the point of a very small needle at a certain *distance* from a metallic plate; this arrangement I prefer to the use of mercury, which always has a certain tremulous motion imparted to it whenever the sparks are produced in the same room in which the inductor is put in motion. But, with an apparatus such as I now employ, when the needle is once fixed, the experiment invariably succeeds (which when mercury was used, was by no means the case), until, perhaps, the point of it is melted off, an incident of frequent occurrence. The most remarkable circumstance attending these experiments is, that the colour of the spark varies with the nature of the current. When, for instance, the positive current passes through the needle to the plate, the spark is yellow or yellowish-green; but when the negative current, on the contrary, passes out of the needle to the plate, the spark is violet.

"This experiment has been repeated very many times without once failing; though sometimes indeed the sparks elicited were so very small as to be hardly visible."

Prof. Steinheil then proceeded to inform the section that an electro-magnetic apparatus from Prof. Gauss, was now erected in one of the rooms adjoining, and regularly used for observations. The apparatus, he went on to state, is used to determine the absolute magnetic variation, and the intensity of the magnetic force, at Munich, and that it is also adapted to measure the perpetual changes which take place in the deflection of the needle.

In accordance with the proposition of Prof. Gauss, simultaneous magnetic observations are now made on fixed days, at various places from Upsal to Palermo, of which Munich is the connecting point between the north and south, and, from the vicinity of the alpine ranges, one of considerable interest. By the kind assistance of several of his friends and colleagues, Prof. Steinheil has been enabled to make regular observations during the two last of these "magnetic periods" of 24 hours each, —that is to say, on the 28th of November 1835, and the 30th of January 1836; the results had been communicated to Prof. Gauss by whom they would be laid before the public. He then showed the section a copy of observations thus made at Göttingen and Munich, graphically reduced; when thus compared, the similarity of the two curves was very remarkable, and evidently must have proceeded from causes operating simultaneously.

This apparatus may also be employed as a galvanometer; for the magnetic bar, which is suspended by a fine wire, is surrounded by a multiplier, by which the most feeble currents are made to produce deflections capable of measurement.

A lever has also been applied to move (with any desired velocity,) the induction-coil up and down along two magnetic bars of 25lbs. weight each; whereby, when in connexion with the commutator, (an instrument similar to the *bascule électrique* of Ampère,) the magnetic bar may be arrested at any point of its deflection.

Prof. Gauss has proposed applying this to telegraphic communications, inasmuch as the bar of a second similar apparatus, whose oscillations are isochronous with the former, is simultaneously and equally deflected, even at very considerable distances, if connexion between them be established by two wires merely.

Prof. Steinheil added that he had fitted up a telegraph here according to this plan, and similar in principle, to that which connects the Observatory and the Cabinet of Natural Philosophy at Göttingen.

Signals made in the room appropriated to the magnetic observations were transmitted to another apartment at a considerable distance, whence the answers were returned to the first room. He had arranged this apparatus for the purpose of demonstrating the peculiarities and the practicability of Prof. Gauss's contrivance, hoping by these means to draw attention to it, and induce persons to employ it for connecting stations far more distant than any to which it has yet been applied.

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Note by Editor.

[During the month of June last year (1836), in a course of lectures delivered at King's College, London, Professor Wheatstone repeated his experiments on the velocity of Electricity, which were published in the *Philosophical Transactions* for 1834, but with an insulated circuit of copper-wire, the length of which was now increased to nearly four miles; the thickness of the wire was $\frac{1}{16}$th of an inch. When machine-electricity was employed, an electrometer placed on any point of the circuit diverged, and wherever the continuity of the circuit was broken, very bright sparks were visible. With a voltaic battery, or with a magneto-electric machine, water was decomposed, the needle of a galvanometer deflected, &c., in the middle of the circuit. But, which has a more direct reference to the subject of our esteemed correspondent's communication from Munich, Prof. Wheatstone gave a sketch of the means by which he proposes to convert his apparatus into an electrical telegraph, which, by the aid of a few finger-stops, will instantaneously, and distinctly, convey communications between the most distant points. These experiments are, we understand, still in progress, and the apparatus, as it is at present constructed, is capable of conveying thirty simple signals, which, combined in various manners, will be fully sufficient for the purposes of telegraphic communication.]

ON DIETETIC CHEMISTRY.

II.

Starch, which is the other proximate element of wheat-flour, is diffused through the water, and gradually subsides in the form of a fine white powder, which, when looked at through a magnifier, appears to consist of small brilliant grains, the size of which varies in the varieties of starch; each of these grains consists of an outer membrane, enveloping a more soluble matter, which has been called *amidine*. When the starch, as originally deposited, has been purified by repeated washing, and drained upon a fine sieve, it gradually forms a cake, which, on further drying in a stove, splits into those small columnar pieces, in which we usually see it in the shops. The greater part of the starch used in this country is manufactured in or near London, and almost exclusively of wheat-meal; but a good and useful starch may also be procured from potatoes, and from a number of other vegetables; and of these varieties of starch, some are used as articles of food, such as arrow-root, sago, tapioca, and cassava. Wheat-starch is chiefly used for stiffening wearing-apparel; and in order to cover the yellow tint which linen is apt to acquire, a little stone blue, or smalt, is commonly added to the starch used for such purposes. Formerly starch was largely consumed in the form of *hair-powder*, and the discontinuance of that absurd and dirty fashion has, of course, materially diminished the demand; in this country, indeed, the use of starch is very limited; but in some parts of Holland, Germany, and Switzerland, where even the lower orders and peasantry wear stiff caps and frills, the manufacture of starch is carried on upon a large scale. There is, however, another outlet for starch, which occasions a considerable demand for that which is of a very inferior quality, and that is, in the manufacture of hard confectionary, such as lozenges, sugar-plums, and similar articles: those which are sold about the streets, and made "for the use of schools," are generally composed of the offal of starch-works, mixed with plaster of Paris, pipe-clay, or chalk, and as little sugar as is able to give them a palatable sweetness; but what is worse is, that they are often coloured with red-lead, verdigris, gamboge, and other mineral and vegetable poisons. A species of *refined liquorice*, manufactured for the same market, is a compound of common Spanish-juice, lampblack, and starch.

The *chemical properties* of starch are well defined; it yields no ammonia when heated in close vessels, and therefore contains no nitrogen[*]. It does not dissolve in cold water, nor in alcohol, but it readily forms a viscid solution with hot water. When a solution of starch is added to one of iodine, a fine blue colour is produced, which is very characteristic of starch, for no other substance produces the same effect. In iodine, therefore, we have a most valuable and ready *test* of the presence of

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[*] Its ultimate elements are—

Carbon	428	equivalent to	Carbon	428
Hydrogen	63		Water	572
Oxygen	509			
	1000			1000

starch; and, on the other hand, a solution of starch is as ready a test for iodine. When starch is roasted, or torrefied—that is, gently and carefully heated over the fire till it acquires a pale-brown colour—it gradually changes its properties, and becomes converted into GUM; it is now soluble in cold as well as in hot water, and it no longer produces a blue combination with iodine. When bread is toasted, it is at the same time superficially *gummified*, and *toast and water*, as it is called, is a solution of the gummy matter so produced. But gum is also found ready formed in very many vegetables; there are, indeed, few without it: it contributes to their nutritive powers, and, when pure, is transparent, tasteless, and forms a viscid solution, or *mucilage*, when dissolved in water. In this state it not unfrequently exudes from the stems and bark of trees, as from the plum, cherry, and apricot; but what is termed *gum-arabic*, furnishes a specimen of this substance in its purest state. Its distinctive chemical characters are, solubility in cold and in warm water, and insolubility in spirit of wine; consequently, when alcohol is added to the aqueous solution, the gum separates, or is precipitated. Gum is also thrown down from its solution by subacetate of lead, with which it produces a compound (of gum and oxide of lead) which is insoluble both in water and in alcohol, but soluble in acids. Large quantities of gum-arabic are brought into this country, chiefly from Morocco. It is the produce of the *acacia vera*, a plant belonging to the class *polygamia monœcia* of the Linnæan system, and to the natural order *leguminosæ*.

There can be no doubt that gum is a nutritious article of vegetable diet, and that it does not, as is sometimes supposed, pass through the bowels unaltered. During the gum-harvest, the Arabs are said to subsist almost exclusively upon it; and it is stated, upon the authority of Hasselquist, that a large caravan of Abyssinians would have starved, had they not fortunately discovered among their merchandise a large stock of gum-arabic, upon which one thousand persons subsisted for two months. Majendie found that when dogs are exclusively fed upon gum, they waste away, and become torpid, losing their appetite in the course of three weeks, and dying in about a month. But no carnivorous animal can be immediately brought to subsist upon food destitute of nitrogen; they require to be gradually inured to the change; and even then, it is doubtful whether any animal can thrive, or even subsist for a long period, if limited to one article of diet. Thus Tiedeman and Gmelin found that geese fed upon gum died on the sixteenth day; those fed on starch died on the twenty-fourth day; and those fed on boiled white of egg, on the forty-sixth day; and in all these cases they gradually dwindled away, and died as if of starvation.

There are many varieties of gum, which, though they closely resemble each other in their ultimate composition and nutritive powers, differ in certain of their properties, especially in the manner in which they are acted upon by water. *Gum-tragacanth*, for instance, or, as it is sometimes called, gum-dragon, when put into water, instead of gradually dissolving into a clear viscid solution, as is the case with gum-arabic, swells up into a kind of opaque gelatinous mass, and requires long boiling before it forms a transparent solution. The gum imported from Bassorah, and the gum which exudes from the cherry-tree, are in many respects similar to tragacanth; hence these modifications have been called *bassorin* and *cerasin*. An analogous gummy substance is found in *linseed*, and in the seeds of the *quince*, both of which yield very viscid solutions when rubbed with warm water. The term *mucilage* is often extensively applied to these modifications of gum. *Salep*, which is the root of a species of *orchis*, exhibits the same characters, and the petals of the marigold and the blossoms of the lime-tree abound in a similar mucilage [*].

There is a substance which was long supposed to be a modification of gum, or a compound of mucilage and some vegetable acid, but which is probably a distinct product, and this is VEGETABLE JELLY. It is contained in many fruits and roots, and is well known, as procured by carefully simmering down the juice of apples or of currants, under the name of *apple-jelly* and *currant-jelly*. This gelatinizing principle of vegetables is distinguished from animal jelly by the absence of nitrogen; and, as it combines with bases, and apparently neutralizes the alkalies in definite proportion, it has been regarded as an acid; it has, therefore, been proposed to distinguish it from gum, mucilage, and animal jelly, by the term PECTIN, or PECTIC ACID, from the Greek word πηκτη, *pekté*, signifying jelly or coagulum.

Pectin is obtained in the purest form from carrots and turnips, especially the former; and the viscid and gelatinous quality of *carrot soup* depends upon its presence: when procured from fruits, it is generally difficult to get it free from colouring-matter; but when carefully prepared from middle-aged carrots, it is quite pure.

The following is the process:—The carrots are scraped, and then rasped, and pressed in a cloth, so as to squeeze out all the juice, which is of a deep-yellow or carrot-colour, and is to be thrown away; the marc which remains in the cloth is then to be washed with repeated quantities of cold distilled water, till it passes off colourless; and when, in this way, all soluble matters have been removed, it is to be dried by pressure. A hundred parts of this prepared carrot is then mixed with about twice its weight of distilled water, to which five parts of bicarbonate of potassa are added; the mixture is simmered for a short time over a gentle fire, and then poured upon a linen strainer, and the liquid pressed out: this liquid is a solution of pectate of potassa; that is, a compound of potassa and pectin, or pectic acid; and when a few drops of muriatic or other acid are added to it, in order to abstract the potassa, the pectin coagulates into a firm tremulous jelly. If it is wished to obtain pure pectin, a solution of muriate of lime is added to the expressed alkaline liquor, which immediately forms a dense precipitate of pectate of lime; this is to be washed with cold water, and then put into water acidulated by muriatic acid, and finally washed again. The pectin may now be dissolved in boiling water, and seasoned or flavoured with sugar, wine, noyau, &c., and so made into an excellent jelly. It has been much recommended in France as a part of the diet of invalids, but

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[*] In ultimate composition, all the varieties of gum closely agree, when they have been carefully reduced to one state of dryness. The ultimate elements of gum-arabic are—

$$\left.\begin{array}{lr}\text{Carbon} \ldots\ldots\ldots & 414\\ \text{Hydrogen} \ldots\ldots & 65\\ \text{Oxygen} \ \ldots\ldots & 521\\ \hline & 1000\end{array}\right\}\ \text{equivalent to}\ \left\{\begin{array}{lr}\text{Carbon} \ldots\ldots\ldots & 414\\ \text{Water} \ \ldots\ldots\ldots & 586\\ \hline & 1000\end{array}\right.$$

the truth is, it is but a poor and weak jelly, and stands low upon the scale of nutritive substances; but it is a very good thickener of meat soups, and renders the carrot an excellent article for that purpose. Pectic acid forms insoluble or difficultly-soluble compounds with many of the metallic salts, and it has been recommended, therefore, as an antidote to the poisonous properties of the salts of copper, lead, zinc, antimony, and mercury; but whether it would be really effective in preventing the deleterious effects of those compounds, seems to be very questionable. We are not as yet accurately acquainted with the ultimate composition of pectin, but it does not contain nitrogen, and probably belongs to the group of organic products which we are now considering, and which are represented by carbon + water[*].

The next, and one of the most important, articles of vegetable diet is SUGAR: it is easily recognised by its sweet taste, and is further distinguished from the preceding proximate principles, by its solubility both in hot and in cold water, and in alcohol; starch being insoluble in cold water, and gum and pectin insoluble in alcohol. There are several varieties of sugar, differing from each other in being more or less easily soluble in water, and more or less crystallizable; but they closely resemble each other in their ultimate composition, which in all cases is represented by carbon + water[†].

The sugar which is consumed in this country is almost exclusively the produce of the *sugar-cane* (arundo saccharifera), a plant which thrives in hot climates. When the cane is ripe, it is cut off at the root, stripped of leaves and ends, and passed twice between the rollers of a mill, so as to express all the juice: to this a small quantity of lime is immediately added, and it is then passed through a series of boilers, in each of which it is skimmed, and to a certain extent purified, till it arrives at the last evaporating-vessel, where it is boiled to *proof*, that is, until it has a tendency to granulate on cooling: it is then transferred into coolers, where it soon crystallizes, and is then put into hogsheads, placed on their ends in the curing-house, with several apertures in their bottoms, through which the *molasses* or *treacle* drains into a cistern below: it remains in

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[*] A very pure form of pectin, or jelly, may be obtained by carefully expressing the juice of ripe white currants, and adding to it about half its bulk of alcohol: in a few hours the jelly separates, and when washed with alcohol and dried, it resembles isinglass in appearance, and when digested in cold water, swells up into a soft pulp like starch, but it is not in the least blued by iodine.

[†] The ultimate elements of pure and dry cane-sugar are—

Carbon	421			
Hydrogen	64	equivalent to	Carbon	421
Oxygen	515		Water	579
	1000			1000

In the Philosophical Transactions for 1827, Dr. Prout has given the results of an elaborate series of experiments upon the ultimate composition of the different grades of sugar, of which the following is an abstract:—

	Charcoal.	Water.
White sugar-candy	428	572
Refined sugar	420	580
Maple sugar	421	579
Beet-root sugar	421	579
Honey sugar	363	637
Starch sugar	362	638

this state till sufficiently dried or drained for package, and is then transferred to hogsheads, and exported under the name of raw or *muscovado sugar*.

Raw sugar is further purified by boiling a solution of it with white of eggs, or with the serum of bullocks' blood and lime-water: during this process the albumen of the blood or of egg coagulates, and, rising to the surface, entangles and combines with some of the impurities of the syrup, which are thus easily removed by skimming. The syrup is then further purified by animal charcoal and other methods, and evaporated till a little of it, taken between the finger and thumb, draws out into threads: it is then transferred to coolers, and stirred till converted (by the breaking down of the crystals as they form) into a granular mass. In this state the sugar is put into conical earthen moulds, which have been well imbued with water, and which are placed with their apex downwards, at which there is an aperture through which the liquid syrup flows out. The crystalline mass is then further purified by suffering water very gradually to percolate through it, which is effected by putting a mixture of pipe-clay and water upon the broad and upper end of the loaf (as it stands inverted); hence the term *claying*, or *clayed* sugar: but a strong solution of pure sugar is now generally substituted for the clay; and inasmuch as the sugar is always more or less damaged by evaporation over an open fire, the heat is applied generally through other media; or the whole of the boiling and evaporating process is so arranged, as to be carried on *in vacuo*, or, at least, under greatly-diminished atmospheric pressure,—a contrivance which enables the operators to get rid of the water at a temperature so low as not at all to injure the sugar*. When thus purified, and when the sugar is sufficiently dry to be taken out of the moulds, it forms common *loaf-sugar*, which is more or less white according to the pains and time which have been bestowed upon that part of the process by which the colouring-matters are removed.

There are no authentic accounts respecting the exact period when sugar was first introduced into England. Even in the early part of the seventeenth century the quantity imported was very inconsiderable; and it was not till towards the end of that century, when coffee and tea were introduced, that it came into general demand. In the year 1700 the quantity consumed was about twenty-two millions of pounds: it now exceeds four hundred millions of pounds, and forms not only a great article of importation and revenue, but an important necessary of life; and even now the demand for it is probably much below its natural limit, and would be increased if the duties upon it were reduced; for it is now charged with the oppressive tax of twenty-four shillings per cwt. on West India sugar, and thirty-two shillings on East India sugar; the average price (exclusive of duty) being from twenty-four to thirty-four shillings per cwt.

In France, and some parts of Germany, considerable quantities of sugar have been manufactured from *beet-root*. The roots are sliced, and the juice expressed, and boiled down to about two-thirds its bulk, with the addition of a little lime. It is said that a hundred pounds of the root furnish upon the average between four and five pounds of purified sugar, besides a quantity of syrup, at the average expense of fourpence per pound. Attempts have lately been made to introduce this manufacture into this country; and a beet-sugar work has been established near London, where, in consequence of the enormous duty on colonial sugar, it is furnished nearly at the same price, that is, at about 10*d*. to 1*s*. per pound. "But as the preservation of the revenue from injury is of infinitely greater importance than the introduction of this spurious business, the foundations of which must entirely rest on the miserable machinery of Custom-house regulations, sound policy would seem to dictate that the precedent established in the case of tobacco should be followed in this instance, and that the beet-root sugar manufactory should be abolished. Inasmuch as it is better to check an evil at the outset, than to grapple with it afterwards, we trust that no time may be lost in taking vigorous measures, should there be any appearance of the business extending*."

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* "A mixture of one part of water and three of sugar boils rapidly at 230°, and from this to 240° is its due temperature: if a solution of sugar be further concentrated by *the evaporation* of water, the thermometer rises to 340°. The sugar then begins to turn black, and at 370° it takes fire, on applying flame to its vapour, and burns strongly, leaving a residuum of charcoal."—*Henry's Chemistry*.

MAPLE SUGAR is the produce of the *sugar-maple* (acer saccharinum), a tree which grows abundantly in the United States and Canada. The sap is obtained by tapping the tree, to the amount of about fifty quarts in the twenty-four hours, and it yields about five per cent. of sugar, which is procured and purified nearly in the same way as cane-sugar.

A species of sugar is also made in some countries from grape-juice; in a good season the expressed juice of grapes yields from thirty to forty per cent. of solid matter, more than three-fourths of which is sugar. This is, however, a weak sugar, and cannot be brought to the hard crystalline state of cane-sugar.

In addition to these natural sources of sugar, there are some methods of obtaining it artificially, and among these, a very curious and interesting one is the conversion of starch into sugar, a process discovered by Kirchoff of Petersburgh, not accidentally, but in consequence of the exclusion of colonial produce from Russia during the enforcement of what was termed the continental system under the reign of Napoleon.

The process consists in boiling starch in water slightly acidulated by sulphuric acid; after some hours it loses its mucilaginous character, and, becoming sweet, is gradually changed into sugar; which may be obtained by neutralizing the acid by chalk, filtering, and evaporating the syrup. The sugar thus obtained resembles that of the grape, or of honey; it is granular, and cannot be crystallized; but it has a pure, sweet taste. The theory of this process is not very intelligible; but on comparing the ultimate composition of sugar with that of starch (as given above), it will be remarked, that they are nearly similar; they are both hydrates of carbon, and a very slight change in the relative proportions of the water and charcoal would suffice to convert one into the other. This transmutation also occurs during the early part of the process of germination; and in the process of *malting* a portion of the starch of the barley is converted into sugar, as we shall afterwards have occasion to observe.

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* *Macculloch's Dictionary of Commerce*, art. Sugar.

The nutritive powers of sugar are probably not very considerable, and what has been said in regard to gum applies also to this substance.

Another most important article of vegetable food, however unpromising it may at first sight appear, is LIGNIN, or WOODY FIBRE. It is true, that wood, or saw-dust, does not sound palatable; but when we consider it, spun, as it were, into those delicate tissues of cells and tubes which form the receptacles of the pulp and juices of our most delicious fruits, sometimes yielding an agreeable and crisp resistance, as in the apple; and at others, melting down in a more attenuated form, as in the beurré pear and the peach; or as forming the more substantial skeleton, as it were, of our eatable vegetables, as in the lettuce, cabbage, French bean, and others; we at once see its claims to a place among esculent, and even nutritive principles.

The accumulation of the elements of which woody fibre is composed, by the organic powers of the vegetable world, is something very surprising: the dry timber of an average-sized oak weighs, for instance, about sixty tons; its durability, and its density in some kinds of timber, is also wonderful; so are its uses and applications in the varied forms into which it is elaborated by the hand of nature; as in hemp, flax, cotton, &c., and the different fabrics into which it is manufactured, such as canvass, linen, calico, paper, and so forth; giving us cables, ropes, thread, &c.

To the chemist all these things appear still more remarkable, when he finds that the woody fibre, or ligneous part of vegetables, is analogous in composition to the preceding *neutral* products, starch, gum, and sugar; and that they are in fact mutually convertible; for woody fibre is a *hydrate of carbon**, regarded in reference to its atomic constitution; so that, assuming, as just stated, that the woody fibre of a moderate-sized oak weighs sixty tons, we have here a *consolidation* of thirty tons of charcoal or carbon, and thirty tons of water.

When woody fibre comes before us as an article of diet, it has other curious and important bearings. If any form of lignin, such as saw-dust, (cleansed from all foreign bodies, such as resin, extractive matter, &c.) rags, or paper, be rubbed up with a little sulphuric acid, taking care that the action of the acid does not go to the extent of charring, and if the acid be afterwards abstracted by adding to the mixture an alkali, or some powdered chalk, it will be found that the *wood* has been changed into a species of *gum:* if we now boil this gum for some hours in acidulated water, (imitating the process for the conversion of *starch* into sugar,) it gradually becomes converted into *sugar*: hay, straw, leaves, shavings, in

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* It appears, from Dr. Prout's experiments, that all the varieties of lignin, or woody fibre, when deprived of adventitious matters, are similarly constituted, the apparent differences depending upon the presence of foreign substances, or upon mere mechanical texture. Two more opposite states of *wood* than box and willow can scarcely be conceived, yet, when purified, pulverized, and dried, they both yielded precisely the same relative proportion of carbon and water: the following is the ultimate composition of pure lignin, deduced from these experiments:—

Carbon	500	equivalent to	Carbon	500
Hydrogen	56		Water	500
Oxygen	444			
	1000			1000

short, any form of *ligneous fibre*, may be similarly converted; and although we do this but clumsily and inconveniently in our laboratories, being, as we are, but Nature's journeymen, Nature herself carries on these transmutations with the most wonderful results, as we see in the ripening of fruits, where the hard woody texture gradually softens down into sweet and luscious pulp, as in the ripening of the pear, the grape, the strawberry, and, in short, almost all fruits*.

Lastly, let us look at the effect of heat on wood. If we burn wood in the open air, it undergoes apparent, but far from real, destruction, as we have already remarked: burned with *imperfect access of air*, its most volatile and combustible parts go off, and its *charcoal*, or at least a considerable part of it, remains: if *distilled*, instead of burned, tar, oil, water, and VINEGAR† are produced; but if *pure woody fibre*, such as beech saw-dust, from which all soluble matters have been carefully washed out, be reduced to a very fine powder, and then cautiously roasted or baked, it acquires characters not unlike those of corn-flour, and when duly mixed with yeast, or leaven, it ferments, and makes an uniform spongy bread, much more palatable than that usually eaten by the peasantry of many parts of Europe, and infinitely preferable to that which is made in times of scarcity from bran and the husks of corn.

Such is a short outline of the history of the WOODY FIBRE, considered merely in reference to its *chemical* properties; and should a subject be at any time required for an essay on the power, wisdom, and goodness of God in the works of the creation, a more prolific or apposite one could scarcely be selected.

How extraordinary, too, do the uses, the applications, and the transmutations of CARBON now appear. We have already adverted to its omnipresence in organic matter, to some of its ordinary forms and modifications, and to the properties by which it is characterized: but, whence comes it in those quantities in which it occurs—whence is it derived for the supply of its prodigious accumulations in the organic world? It forms, as we have seen, half the weight of dry timber, and of all the varied forms of woody fibre; and enters, in about the same proportion, into the composition of our vegetable food. Consider the composition of *sugar* only, without entering into the gigantic calculation that would apply to *bread:* about half the weight of sugar is charcoal: now Europe, *Europe only*, consumes about 500,000 tons a-year, the prime cost of which, if we look at it in a financial point of view, is twelve millions sterling; or, with the duty added, twenty-one millions; so prolific a source is it of our revenue: to constitute this sugar, the vegetating cane would require 250,000 tons (calculating for Europe only,) of carbon,

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* See Berard's table, showing the relative composition of fruits at different periods of their maturity, in the *Annales de Chimie et Physique*, tom. xvi., or Brande's *Manual of Chemistry*, 4th ed., p. 877.

† The analogy between wood and vinegar (or rather acetic acid) is very remarkable; the ultimate composition of acetic acid being as follows:—

Carbon	470	equivalent to	Carbon	470
Hydrogen	59		Water	530
Oxygen	471			
	1000			1000

and 250,000 tons of water, or its elements: for the water we can perhaps easily account, but whence the carbon? It comes either from the soil, that is, from various forms of manure, or from *carbonic acid* in the atmosphere, and perhaps chiefly from the latter. But all this charcoal has either already gone the round of life in our own time, or it is of a more remote origin: and there can be no doubt that a great source of the carbon which we thus search for may be traced to the *combustion of coal;* so that the elements which once formed the forests of the primæval globe, torn from their native soil, and buried by the storms and inundations of ancient epochs, long lay dormant in the form of coal; this, rendered accessible to man by the upheaving force of subterranean fires, becomes subservient to our wants and conveniences, to our knowledge and power; and reassuming, in this stage, its gaseous character, and receiving wings, as it were, which carry it to the remotest parts of the globe, it passes through the medium of the atmosphere into the vessels of plants, and is there elaborated into their products, and into sugar, among the rest. It is apparently to these accumulations of carbon in our coal-strata, which during a long succession of ages lay dormant and inactive, that we are in part, if not chiefly, indebted for the supplies required for the present order of things, and for the demands of the increased population of the globe: and how strenuously does this fact enforce and illustrate the argument of design, foresight, and providence upon which the natural theologian insists, and which every step in experimental science sanctions and supports*!

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* Dr. Prout and Dr. Buckland have each, in their *Bridgewater Treatises*, adverted to the extraordinary characters of the carbonaceous element, the former considering carbon chiefly in reference to its abstract chemical properties, and the latter to its existence in coal, and to the effect of mineral fuel upon the actual condition of mankind; showing that it is, in an especial degree, the foundation of increasing population, riches, and power; and of improvement in almost every art which administers to our necessities and comforts. But they might each have carried their arguments upon this curious subject much further, by enlarging upon the suggestions in the text, and by pointing out the extraordinary conditions of this element in its various states; its fixity and indestructibility by heat alone, its insolubility in all menstrua, its inert and passive characters; and yet, when combined with the oxygen of the atmosphere, acquiring the extreme mobility of air itself, becoming soluble in water, and poisonous when respired, or when retained in the blood.

MECHANICAL PHILOSOPHY.

The Mathematical Principles of Mechanical Philosophy, and their Application to the Theory of Universal Gravitation; by John Henry Pratt, M.A., *Fellow of Gonville and Caius College, and of the Cambridge Philosophical Society.*

Though we cannot give our readers so complete an account of this work as we could wish, owing to the restrictions they have imposed on us of not entering into purely mathematical investigations, yet we can present them with an outline of its contents and arrangement; and this is the more necessary, since no one could form a guess, from the title, either of its object, or of the admirable manner in which that object has been attained.

The necessity for uniformity or consistency of method in studying any branch of knowledge, must be in proportion to the difficulty or extent of the subject; and there can be none more imperatively requiring it than mechanical philosophy, in its vast and increasing dominion. If many of the elementary results of this branch of mixed mathematical science may be arrived at by the application of purely *geometrical* methods, the investigations of the minute and complicated perturbations in the planetary motions, caused by their mutual action, can only be carried on by the application of the most refined analysis: now, since this is also applicable in the former case, analysis should be made use of throughout, for the sake of that unity especially essential in a study the inevitable difficulties of which are sufficiently numerous without the addition of such as are superfluous; and those must be so considered that compel any mental exertion not tending to the immediate object in view. We think that so much will be conceded by those who regret the gradual banishment of pure geometry, and the substitution of analysis, in the higher branches of mathematics: we are therefore saved the necessity of entering here into the *quæstio vexata* of their respective merits; and having in an early part of our work briefly touched on this subject, we will only remark in addition, that the regrets alluded to partly arise, as we suspect, from the indolence which deprecates new labour, partly from captious jealousy of *innovation*, even in science, but most frequently from an inadequate notion of the relation and limits of geometry and analysis.

An attachment, and that in most respects a laudable attachment, to the geometry of the *Principia* had, till of late years, led to the practice of retaining in our course of University reading some parts of that immortal work, rather for the beauty and elegance of its demonstrations, than for the importance of the theorems demonstrated. But this practice has been gradually sinking into disuse, a result which we owe to Professor Woodhouse's *Physical Astronomy*, to M. Poisson's *Traité de Mécanique*, which has been extensively used amongst us, and very largely to Mr. Whewell's Treatises on *Statics* and *Dynamics*, and Mr. Airy's *Mathematical Tracts*. But notwithstanding the great and happy changes thus brought about, we still cling to the old methods, not as a whole, but just so far as to derange our system, and give to it the ambiguous character of being neither strictly geometrical, nor strictly analytical. But I wish not to be misunderstood: I mean not to imply that geometry should be discarded and banished from our academical course of study; far from it; for the analyst will find his analysis of little benefit if he have not the power of gathering from his formulæ geometrical conceptions. Neither would I have it for a moment conceived, that I would in the least degree repudiate the profound veneration which is so justly due even to the letter of the *Principia:* my own admiration of the clearness and conciseness of its demonstrations rather induces me to invite others to participate of the pleasures they may enjoy from its attentive and diligent study. But this I desire, that we should pay more regard to system than we hitherto have done; if our course is to be geometrical, let us adhere to geometry; if analytical, to analysis; if we are to admit both (the preferable course), let us keep our systems well apart, and not have our course of reading confused, here analysis and there geometry.—*Preface*, p. iii.

In accordance with these views, Mr. Pratt has produced a complete course of mechanics possessing this desirable object of unity of method; and while he has rendered it, mathematically considered, *teres atque rotundus*, he has superadded, occasionally, observations and illustrations that confer additional interest on his work. In a brief Introduction, containing general principles and definitions, the division of the subject is stated thus:—

MECHANICS is the science which treats of the laws of rest and motion of bodies, whether solid or fluid. It is divided into four branches:

1. STATICS, which treats of the laws of the *equilibrium* of solid bodies.
2. DYNAMICS, of the laws of *motion* of solid bodies.
3. HYDROSTATICS, of the laws of equilibrium of fluid bodies; and
4. HYDRODYNAMICS, which treats of the laws of motion of fluid bodies.

These sections are subdivided into chapters, in which the student is conducted, by a series of propositions, from elementary principles up to the investigation of the most complicated planetary motions, the demonstrations and analyses being worked out with equal precision and elegance[*].

We shall now proceed to give a few passages, with remarks on them, in the desultory order they occur in turning over the pages.

At the conclusion of the first chapter, we find a demonstration of the principles of *virtual velocities*, a principle which is thus enounced: "If any number of forces acting on a particle be in equilibrium, and the point of application be moved geometrically through any small space, then the sum of the products of the forces, and the spaces described by the point of application, relatively to the direction of the forces, will vanish; these spaces being reckoned positive when drawn in the direction in which the force acts, and *vice versâ*." In the third chapter this principle is shown to hold good of any system of forces keeping a rigid material system in equilibrium; and in p. 49, La Grange's mechanical, or empirical, proof of the principle is explained, to show that the whole doctrine of the equilibrium of a rigid system might be deduced, by starting from this principle, by an inverse mode of proceeding.

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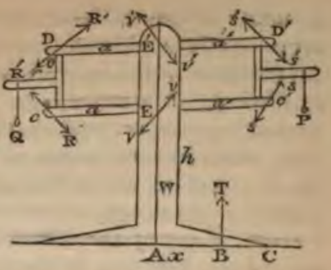
[*] Mr. Pratt adheres to the old differential and integral notation, chiefly, as he says, on account of the merits of Fourier's notation for definite integrals.

The proposition that two equal and opposite parallel forces do not admit of a resultant, which is usually proved geometrically, as an extreme case of the parallelogram of forces, is here analytically demonstrated, the value of the resultant becoming $= 0$, and the distance x of its point of application from that of either force $= \infty$. The series of propositions which follow on the properties of *couples*, as such a system of forces is called, places the advantages of the analytical treatment of the subject in a striking point of view.

Chapter V. is devoted to "Machines and Friction;" it opens with this definition, "A machine is an instrument, or a system of solid bodies, for the purpose of transmitting force from one part to another of the system." The simple mechanical powers are, properly, brought under this definition, and termed machines. This would have been a proper opportunity for entering somewhat at large into the mechanical philosophy of the simple machines, and of explaining that they only enable us to apply a given force more advantageously; it sounds very paradoxical to a beginner to be told that no force is produced by the lever which enables him to move a weight he could not stir by his unaided strength. The explanation lies in a correct discrimination between *force* in its mathematical sense, and *power* in the common acceptation of the words; and in such a work, even a refined and ample disquisition on the subject would not have been misplaced.

In the account of the balance, we meet with the following:—

> There is a remarkable balance, called, after its inventor, Roberval's Balance: a representation of it is given in fig. 35. DC is a frame of which the opposite sides are equal, and the extremities are connected by pins at D, C, D', C', so as to allow of free motion: this frame is supported by a stand $EE'A$, being connected to it by pins at E and E' so as to allow of free motion about those points: EE' must be parallel to DC and $D'C'$, but not necessarily equidistant from them: arms are fixed at right angles to the sides DD', CC' to support weights Q and P. The peculiarity of this machine is, that if P and Q balance in any given position on the horizontal arms, the equilibrium will remain undisturbed if we shift P or Q, or both of them, along their arms in either direction: also if we push one arm down, and consequently raise the other, the whole will remain at rest in the position in which it is left. We shall prove these facts, and explain the paradoxical character of the machine in the Chapter of Problems. We may, however, easily prove by the Principle of Virtual Velocities the facts mentioned above, though the paradox will not be removed.
>
> If we lower the arm on which P acts through a space x, then D' sinks through a space x, and D, and therefore the arm on which Q acts, rises through a space $\frac{a'x}{a}$, which is independent of the distances of P and Q along their arms: a and a' are the lengths DE and ED'.
>
> $$\text{Then } P.x - Q.\frac{a'x}{a} = 0 \text{ by Virtual Velocities,}$$
>
> $$\text{or } \frac{P}{Q} = \frac{a'}{a},$$
>
> *for all positions of* the frame and of the weight



It will be seen, upon referring to the Chapter of Problems, that although the equilibrium remains undisturbed when *P* and *Q* have different positions, yet the strains at the joints *D*, *D'*, *C*, *C'*, *E*, *E'*, and the point of application (*B* in figure) of the downward pressure, undergo changes.

It is on the principle of this machine that the balances used of late years in shops are constructed; the scales rest each by one point upon the extremities of a lever below them, and the only motion they are capable of is in a vertical direction.

Mr. Pratt has spared but little space to the subject of friction; but this, perhaps, is consistent with the purport of his work: he gives three of Coulomb's laws, but without comment, and refers to the original essay. If P be the normal pressure of a body on a surface, then the friction is $= \mu P$, where μ is a constant quantity for the same materials, and is called the *co-efficient of friction.* By experiment, it is found that

$\mu = \frac{1}{2}$ surfaces being wood, the grain in the same direction,
$= \frac{1}{4}$ ditto ditto in the opposite ditto,
$= \frac{1}{4}$ metallic surfaces,
$= \frac{1}{5}$ one surface wood, and the other metal,

when the surfaces are of finite extent, and they are in the state bordering on motion.

Our readers may perhaps be aware that Mr. Rennie instituted some experiments by which he determined the co-efficient of friction for a variety of different materials, as well as the *limiting angle of resistance*, or the greatest angle which the direction of the mutual pressure of two surfaces in contact may make with their common normal, before one begins to slide on the other.

It is not commonly explained in works on practical mechanics, that friction is only a result of the impenetrability of matter. Since it is impossible to reduce two surfaces to perfect planes, owing to the texture of the material of which they are formed, every surface, however smooth it may appear, is covered with innumerable elevations or depressions, which, when the surfaces are in contact, become mutually engaged in each other, and resist lateral motion, till the force applied in a direction parallel to the general one of the surfaces is sufficiently great, either to grind away the inequalities, or, when decomposed into two, of affording one which is adequate to overcome the force of pressure or gravity, and to raise the upper surface free of the inequalities which opposed that lateral motion. It is probable that the heat produced by friction may be that disengaged by the rugose particles on being violently condensed, before they are absolutely displaced, or ground away. The effect of grease, or even of a fluid, in diminishing friction, is produced by the substance filling up in some degree the depressions in either surface, and thus preventing them from coming into direct contact: the inequalities on the grease, of course, yield readily to lateral force.

It may be easily conceived that the degree of smoothness to which a surface of any material can be brought, as well as the nature and degree of those ultimate inequalities that elude our senses, depend on the difference of the material, and hence the difference in their co-efficients of friction. It must also be recollected, that when two surfaces are working together in contact, the friction detaches particles from one, or both, which by their action on the surfaces modify them still further; if the two surfaces are of the same material, these detached particles act equally on them, and, unless they can be removed, tend to increase the friction; if one of the surfaces is softer than the other, the particles are pressed into that one, by the pressure of the other, and produce a rougher surface than would otherwise exist, which rougher surface rapidly acts on the harder material. This is the explanation of the apparent paradox, that when hard steel works in brass the steel wears faster than the softer metal.

In the sixth chapter, Mr. Pratt treats of roofs, arches, and bridges, and investigates the mathematical expressions of tension, &c. in timber and iron frame-work, and the conditions of equilibrium and pressure in stone arches; and then explains the structure of the roofs, towers, and domes of a few celebrated buildings. There is also a series of propositions on suspension-bridges, the results being tabulated, and the application illustrated by examples. The whole chapter is extremely interesting, in showing how practical rules are immediately deduced from mathematical theorems; but it is too brief and too general for the merely practical man, who, of course, would neither have recourse to such a work, nor could expect to find in it the kind of full information he requires.

The following chapter is supplemental to the two preceding it, and contains a collection of statical problems; among them is the determination of the conditions of equilibrium in Roberval's balance, which we before alluded to. In chap. viii., on attractions, we come to the preliminary portions of physical astronomy that fall within the first division of the work; these consist of the investigations of the attractions of spherical and spheroidal bodies, &c. We may here, once for all, observe that Mr. Pratt's arrangement and mode of treating that most interesting of all subjects, the laws of planetary motions, appears to us to be exemplary, and superior to that adopted by Mrs. Somerville in her *Mechanism of the Heavens*. This comparison is unavoidable in speaking of any work on the subject, since they must all be more or less based on *La Place's*, and therefore mutually recall the remembrance of each other. The superiority we allude to consists in a more connected arrangement of the propositions, each being dependent on, and immediately deducible from, those that precede it. The investigations and demonstrations are given with more precision and elegance, and the student's attention is not diverted, nor the chain of mathematical reasoning broken, by disquisitions more immediately relating to practical astronomy, or to the simple phenomena of the heavens. Mrs. Somerville aimed at making her subject more *popular*, and at enticing others to a participation of the refined pleasures afforded by those profound studies in which she is so distinguished: she therefore sacrificed to this laudable object some of that method and more scientific mode of treating it, that her own taste and knowledge would otherwise have led her to prefer; we fear, however, that she has failed, and that her work is but little *read*, great as are its merits. Mr. Pratt, on the other hand,—with a conviction in which we fully participate, that no branch of mathematics requires any extraneous allurements for those who have energy and industry to qualify themselves to share in its pleasures, while nothing will ever induce those who have not, to make the exertion,—has addressed his work only to the elect *, by whom it will be fully appreciated. The division on Dynamics opens with introductory observations on motion, velocity, &c.; and we here catch a glimpse of the school in which the author learnt *practical* mechanics. The passage is a favourable specimen of his powers of familiar explanation:—

> We have a popular illustration of the effects of forces which act for a finite time, and for an indefinitely short time, in the game of cricket. The bowler rotates his arm in order to give the ball velocity,—he opens his hand and the ball flies from him with the velocity acquired, and (supposing he deliver the ball *full pitch*), after moving in a curve slightly deflected downwards by the Earth's attraction, is received upon the bat. Now this velocity was generated by the muscular effort of the bowler's arm acting on the ball during the finite time that he retained it in his grasp. While this is going on, the batter swings his bat that it may acquire a great velocity; and the ball and bat come in collision: and what is the consequence? the ball flies back; not only is its original motion destroyed, but new motion is given to it, as if instantaneously, in an opposite direction.
>
> We explain the phenomenon of this sudden recoil in the following manner. When the ball and bat come in contact, their particles are moving in opposite directions, and tend to penetrate each other: but the molecular forces by which the particles of each of the bodies are bound together are too powerful to allow of this separation; nevertheless, the relative positions of the particles are slightly changed by the yielding of the bodies, and, in consequence of their unnatural restraint, a mutual resultant pressure is exerted by the bat on the ball and by the ball on the bat, till their relative motion is destroyed: but the particles of the two bodies are still under restraint when the motion is destroyed, and the mutual pressure of the bodies now acts to effect their separation, and new velocity is generated: this process, which we conceive represents the actual process in nature, goes on with inconceivable rapidity, in consequence of the great intensity of the molecular forces which bind the particles of each body together. If the bat split or the ball burst, then the molecular forces which held together those particles which separate were not powerful enough to resist the separation. It is evident that we are concerned not with the changes which take place during the collision, but the whole change produced.

The laws of motion being analyzed in the first, the reader is conducted in the following chapters, to vi. inclusive, through the investigations of the planetary motions and perturbations; and in chapter vii. we descend again from the heavens to the more abstract dynamical investigations of the motion of a particle in curves and on surfaces, and of the properties of the pendulum, &c., which furnish new data and new starting-points to enable the writer to complete the planetary theories; but from all this, which constitutes the greater part of the work, we can find no passage which will admit of being isolated and laid before our readers, except the following, which will excite surprise in those who do not immediately see the connexion, at being met with in such a work:—

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* Our readers must not be misled as to the nature of Mr. Pratt's work: he did not profess to write one that should be mathematically elementary, in the popular sense of that term, but one which should be complete. No one can proceed ten lines in the perusal of it who is not thoroughly master of the higher analysis.

> The degradation of rocks and the consequent action of collision which is incessantly taking place in large portions of matter on the surface of the earth, the unceasing action of waves on the sea-shore and the collision of the waters of the ocean upon the solid nucleus of the earth, and other like causes, are continually causing a loss of vis viva in the earth's mass, and if allowed to act without any compensating phenomena would in the course of time produce a sensible effect in the length of the day; but on the other hand the explosions of volcanoes are compensating causes. Also the downward motion of rivers, the descent of vapour and cloud in the form of rain, the descent of boulders and avalanches, and various other causes, all tend to remove large portions of matter nearer to the earth's centre, and would in the course of time produce a sensible increase in the length of the day, since we have seen that the vis viva of the earth is constant, if we neglect the attraction of the sun, moon, and planets, and consider only the action of finite forces. But the ascent of vapour by evaporation, and the effect of earthquakes and volcanoes in removing masses of matter to a greater distance from the centre, have an opposite effect. On the whole, all these causes balance each other, since observations have shown that the length of the day has been invariable for many ages.

The section on Dynamics concludes, as did the former, with a collection of problems[*].

In the division on Hydrostatics the figure of the earth is treated as a body of revolution consolidated from a fluid state, and then the subject of tides is discussed partly in this section, as far as the equilibrium of the ocean is concerned, and partly in the following one; but both the sections are dismissed in a summary manner, the principal subjects to which they apply being foreign to the purport of the work. In the "conclusion," Mr. Pratt has summed up the general arguments in proof of the theory of universal gravitation. The following passage puts a fact in a striking point of view, and illustrates the nicety to which direct observations on planetary motions have long attained, and the stupendous powers of that language which can assign values and detect the laws of such comparatively minute deviations.

> Now the calculations of the position of the planets made upon this hypothesis of their gravitating towards the sun with a force directly as their mass, and inversely as the square of their distance from the sun, are found to agree very well with the observed positions, if the calculations extend over only a few years. After the lapse, however, of a considerable interval of time, as a century, minute errors are detected in the calculations, and they are then found not to agree exactly with the observed positions of the planets. But this disagreement is in fact precisely what we should have anticipated, since, if the principle of gravitation be universal, the planets would attract each other, and consequently disturb the elliptic motion and the equable

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[*] We would just hint to the author that all these problems, with their solutions, would form a very pretty amusing book by themselves for idle hours.

description of areas; likewise the deviation of the figures of the planets from perfect sphericity, and their heterogeneous structure, give rise to additional errors. An idea of the extreme smallness of the perturbations may be learned from the fact, that if we trace on paper an ellipse ten feet in diameter to represent the orbit in which the earth is moving at any instant about the sun, and if we trace by its side the path actually described in its revolution round the sun, the difference between the original ellipse and the curve actually described is so excessively minute, that the nicest examination with microscopes, continued along the outlines of the two curves, would hardly detect any perceptible interval between them: Herschel's *Astronomy*.

We must here conclude our notice of this work, much regretting that we cannot enable our readers to participate in the pleasure and gratification the study is calculated to impart. It cannot be expected that anything very new can be brought to light on a subject that has occupied, and is occupying, the attention of every mathematician capable of grasping it; nor does Mr. Pratt lay claim to any other merit than that of bringing before the student a complete and uniform body of investigations on the most exalted subject in physical science, and of divesting it of all unnecessary difficulties. What he intended, he has, in our opinion, most successfully performed; and the book and its publication is equally creditable to the author and to the state of mathematical science of the country. Next in value to the enviable power of originating those discoveries which have made Newton, Le Gendre, La Place, and many others immortal, is that of being able to follow their reasonings in their all-powerful language, and the taste to appreciate and admire them.

A POPULAR COURSE OF ASTRONOMY.

No. IX.

THE SIDEREAL YEAR.—THE ANOMALISTIC YEAR.—THE TROPICAL YEAR.

It is a source of great perplexity in astronomy, that none of those systems of lines which are imagined to be described in space, and none of those points to which are referred our admeasurements of the positions of bodies upon the celestial sphere, are in reality *fixed*.

To begin with the earth's orbit; it is found that by reason of the disturbing attraction of the other planets composing our system, the earth does not, year after year, describe the same path in space. If observations be made determining the position of the line of the apsides, or the line joining the aphelion and perihelion, of the orbit in which it is moving in one year, these observations being repeated the next, will show that it is then moving in an orbit, the line of whose apsides* differs in direction from the former by an angle of 11·8″ eastward. The longitude of the perihelion was, on the 1st of January 1801, 99° 30′ 5″, and during the thirty-four intervening years, it has increased thirty-four times 11·8″, so that now it is 99° 36′ 46·2″. The time after which the earth returns into the same position in space with respect to the sun, or after which the sun appears to return to the same point in the heavens, is called the Sidereal Year: its length is 365ᵈ 5ʰ 9′ 9·6″. The time intervening between two successive passages of the earth through an aphelion, or perihelion, of its orbit, is called an Anomalistic Year; it is greater than the time it would require to bring the sun apparently to the same point of the heavens by the time of describing 11·8″; the length of the anomalistic year is therefore 365ᵈ 6ʰ 13′ 53·5″.

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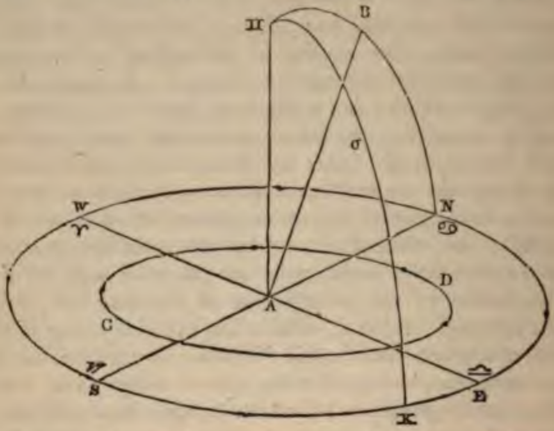
* The perihelion and aphelion of a planet's orbit are called its apsides.



Were the position of the earth's axis always parallel to itself, the position of the plane A Π N, would always be the same; and thus the positions of the points ♋ and ♑, and also ♈ and ♎, dependent upon this plane, would always be fixed; so that the longitude of any fixed point in the heavens, as for instance a star, which longitude is the distance measured upon the ecliptic of that point from ♈, would always be the same. But the earth's axis is not always parallel to itself; it varies continually its inclination, according to a law dependent upon the rotatory motion of the earth about its axis, and the attraction of the sun upon the spheroidical excess about its equator. This variation in the parallelism of the earth's axis, brings with it a change in the position of the line A B in the figure, and of the plane A Π N, and ultimately of the position of ♈, which point is thus made to move 50·1″ every year backwards, or in a direction opposite to that of the earth's motion. This annual regression of the equinoctial point ♈, and therefore of the opposite point ♑, is called the Precession of the Equinoxes.

It is manifest that the longitude of a star increases every year by the amount of this precession, not by reason of any motion of the star, but by reason of a falling backwards of the point from which it is measured. Moreover, that for the same reason the earth does not return to its equinoxes after the same period which it takes to come back to the same point in space, but in a period less than this by the time of describing 50·1″ of longitude. The time of returning from equinox to equinox, or from tropic to tropic, is called the Tropical Year; its length is *365ᵈ 5ʰ 48′ 49·7″.*

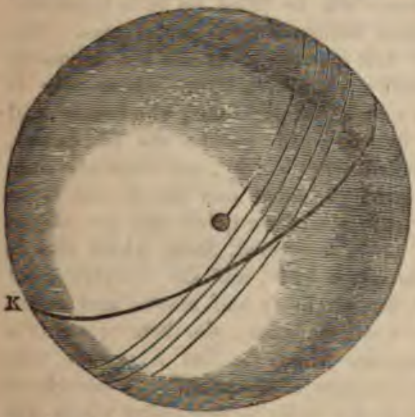
THE MOON.

LIKE the sun, the moon has an apparent motion from west to east amongst the fixed stars. In tracing her path, however, we encounter none of those difficulties which beset the investigation of the ecliptic, or sun's path in the heavens. The stars are, many of them, visible by moonlight; and as these are fixed in the heavens, and their places known, we can tell accurately the position of the moon at any time, by measuring her angular distance from any two of these. Setting off these distances on the celestial globe, her exact place on it will be known, and this being done from day to day, her path will be traced out. Her motion is so exceedingly rapid amongst the stars, that she may almost be seen to move among them; describing somewhat more than half a degree every hour. She thus makes a complete circuit of the heavens in a mean or average period of $27^{d}\ 7^{h}\ 43'\ 11{\cdot}5''$, called a periodical lunar month. During four or five days of each such period she is invisible; and she always becomes thus invisible when her position in the heavens is within an angular distance of about 30° on either side of the sun; she first appears under the form of a slender crescent, or semicircle of light, about 30° above the western horizon at sunset; her angular points, or horns, are then turned towards the left of the spectator, her motion being eastward, at the rate of about 13° through the heavens in twenty-four hours. The sunset of the next evening finds her somewhere about 12° further from the sun, who has moved in that period about 1° in the same direction; thus her elevation above the western horizon at sunset is now about 42°; her crescent will be found, from a mere line of light, now to present the appearance of a lune, as it is termed, of considerable breadth, and in four or five days more, when she has attained a distance of 90° from the sun, and passes the meridian at sunset, the space between her horns will be filled up with light, and her disc will have become a complete luminous semicircular area. She is then said to have completed her first quarter. As she still advances towards the east, this semicircle will swell into a figure whose edge is still a semicircle, but whose base is now an elliptic lune; she is now said to be gibbous, and in her second quarter; this quarter she terminates, when her distance from the sun has increased to 180°, and when she rises about sunset. Her disc is now a complete circle, and she is said to be at her full. Still continuing her journey eastward, her full orb begins to contract on that side, or limb, as it is termed, from which she is moving, until her distance from the sun is 270°, and she completes her third quarter, when she is again a semicircle, or half moon; and in her fourth, or last quarter, she wanes until, as before, she becomes only a thread of light, and finally disappears when she has a second time approached the sun to within the distance of about 30°. When she is in the middle of that portion, 60°, of her orbit in describing which she is invisible, she has the same longitude as the sun; and the instant when this occurs, is called the time of new moon. These different appearances of the moon are called her PHASES.

Although certain portions of her disc are thus, to the naked eye, always, except at the full, obscured, yet may the whole disc at any time be *distinguished by the* aid of a telescope,—dark indeed, but yet not so dark as the surrounding sky*; this obscured disc of the moon is most easily seen towards the time of new moon, and especially about the third day from it. As seen through a telescope, that edge of the moon by which it is waxing or waning presents always an exceedingly ragged appearance, and occasionally bright spots may be distinguished at a short distance from it, in the otherwise obscured part of the disc. The appearance of the disc itself, under the telescope, is exceedingly varied; it is covered with irregular markings, of which the light is dimmer than that of the rest, and which have the appearance of stains. Of these, some are manifestly the shadows of mountains; in other places may be traced the appearance of cavities, commonly of circular forms, on the sides of which lying towards the sun are dark shadows; by measuring the widths of which, the depths of these cavities may be estimated. The same method of measuring the width of the shadow serves to ascertain the heights of the lunar mountains; these are, some of them, about 1¾ miles in height, and the depth of the deepest cavities may be about the same.

If the path of the moon be accurately observed and traced during any one revolution on the celestial sphere, it will be observed—First, that the line along which she moves through the heavens and among the stars is not very far removed from the path of the sun, but that it does not accurately coincide with it; being in point of fact, inclined to the ecliptic at an angle of 5° 8′ 48″.

Secondly, that she does not return, after each revolution, exactly to the same point of the heavens from which she set out, so that no two successive revolutions are made through exactly the same path in the sky. She does not, in fact, describe a great circle of the celestial sphere, but a continuous spiral, which, at every intersection, is inclined at the same angle of 5° 8′ 48″ to the ecliptic. The points of this spiral orbit where she intersects the ecliptic are called her nodes; and when she thus passes from the southern to the northern side of the ecliptic, she is said to be in her ascending node; when from the northern to the southern, in the descending node. Were her apparent orbit a great circle of the sphere, these nodes would be exactly 180° from one another, but by reason of the spiral form of her orbit, whence results a continual deflection of her path from the plane of the great circle on which she first set out, she is brought to cut the ecliptic before she has completed her circuit of 180°; and setting out again from this node, she again comes to the ecliptic before she has travelled 180°. Thus, then, leaving her ascending node, or ascending north of the ecliptic,



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* By reason of the light reflected upon it from the earth, and called the earthshine.

to her greatest distance from it, then passing beneath the ecliptic and to her greatest depth beneath it, and ascending again, she finally crosses the ecliptic at an ascending node, a second time, before she has completed 360°; or the ascending node has, during this revolution, fallen back, as it is termed, upon the ecliptic. Now this occurs at every revolution; there is, therefore, a continual regression of the moon's nodes upon the ecliptic—precisely analogous to that of the equinoctial points in the sun's path, called the precession of the equinoxes. Although these phenomena have thus a resemblance, yet do they arise out of very different causes, and the amount of the regression of the moon's nodes is very different from that of the equinoctial points. The annual amount of the regression of the equinoxes on the ecliptic is 50·1″, one degree in 71·8563 years, or a complete circuit of the ecliptic in 25868 years. The regression of the moon's nodes upon the ecliptic is at the rate of 3′ 10·64″ a day, or one degree in about nineteen days; 19·3286° in a year; and 360° in 6793·39 mean solar days, or about 18·6 years. When the nodes have thus almost 360°, or a revolution through the ecliptic, they commence another, and the moon retraces the spiral path in the heavens which it had commenced 18·6, or nearly nineteen, years before. This path of the moon described in 18·6 years, if traced upon the celestial sphere, would be found almost to cover, with a series of intersecting lines, a belt or zone of somewhat more than 10° in breadth, having the ecliptic in its centre. In the period of 18·6 years, the moon then passes at least once over every point in this belt*, and of the stars which lie in it, which, as we shall shortly see, are all of them infinitely more remote than the moon; it occults or hides from our view, at some time or another, every one. And the sun too, which moves (or whose appearance in the heavens is the same as though it moved,) sluggishly along the centre of this belt, must evidently once, and in reality more than once, have the centre of the moon pass nearly over the centre of his disc, and there must be very many positions of the moon in which her disc must *partially* intervene between us and that of the sun. It is manifest, that knowing the exact motion of the centre of the moon in her orbit, and knowing the exact motion of the centre of the sun in the ecliptic, we can tell when their discs will interfere with one another; for we can tell when the distance of their centres is less than the sum of their radii; whenever this is the case, their limbs must be superposed. It is under these circumstances that a solar eclipse takes place, and it is thus that the time of a solar eclipse is calculated.

It is manifest that, since the moon describes again the same path always after 18·6 years, passing over any star, she will return after 18·6 years, and pass over or occult that star again, as she did before: in respect, then, to the occultations of fixed stars, 18·6 years may be considered as the cycle of the moon. If the sun remained stationary in the heavens as do the fixed stars, after the moon had in any way eclipsed it, it would return and eclipse it again, precisely in the same way, and to the same degree, when this cycle of 18·6 years had expired. But the sun is *not* fixed in the heavens; he moves about one degree daily, and describes 360° in one mean solar year. He will not then, like the moon, in 18·6 years have arrived again at the place in his orbit from which he set out. But nevertheless, by a very remarkable coincidence, at the end of the nineteenth year, that is, $\frac{4}{10}$ of a year only after the expiration of the cycle,—when the sun is, of course, in the same place in the heavens as he occupied nineteen years before, and the moon is revolving in a circle or turn of her spiral path (see the preceding figure,) which is but a short distance from that in which she was revolving $\frac{4}{10}$ of a year before at the expiration of her cycle, and, therefore, very little different from that in which she was revolving at its commencement, nineteen years before,—her longitude will be precisely the same as it was at the commencement of the cycle. Thus describing nearly the same path in the heavens, and having the same longitude, or being at the same point in that path, she must occupy very nearly the exact place in the heavens which she did nineteen years before; the sun, moreover, occupies the same place; whatever relative positions they had then, they must, therefore, have now, and if there was an eclipse then, the same eclipse must occur now.

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* *The width of the* moon's disc is here taken into account.

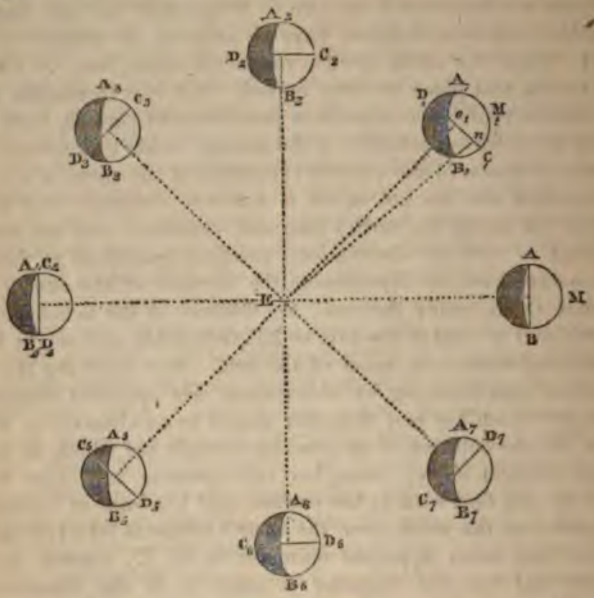
It is easy to ascertain that the longitude of the moon will be the same nineteen years hence, as it is now. The moon will return once into the same position with respect to the sun in longitude that it has at the present instant, that is, it will complete one synodic revolution, in 29·530 5887 days. And it will have returned into the same position with respect to the sun 235 times, after 235 such synodic revolutions, or after 235 times 29·530 5887 days. Now this period of days is precisely nineteen tropical years; but, after this period, the sun will have returned nineteen times to the same place in the ecliptic. The sun is, therefore, in the same place, and there is the same difference of longitude between it and the moon as nineteen years before. And moreover (and this is the chief point), the moon is describing very nearly the same turn or circle of her apparent orbit, so that her latitude at that time is very nearly the same as nineteen years before, and, therefore,—her latitude and the difference of her longitude and that of the sun being the same,—the distance of her centre from his is the same.

The discovery of this cycle, in which all the appearances of the moon in respect to the sun return in the same places in the heavens, and at the same dates of the year, including even the eclipses, is attributed to Metho; it was adopted by the Athenians 433 years B.C., to regulate their calendar, the dates and festivals of which were made partly to depend upon the appearances of the moon. They engraved the number, and the process of the computation of it, in letters of gold on the temple of Minerva, and thus it was that the number which marks the position of any year in the lunar cycle of nineteen years, came to be called the Golden Number.

Such are the appearances of the moon in the heavens; appearances which any one may verify by direct observation. Now it is the object of that branch of Astronomy called Plane Astronomy, of which we are now treating, from the apparent motions of the heavenly bodies to educe their true motions. What, then, is the real motion of the moon? She appears to revolve round the earth in a path very nearly coinciding with the ecliptic; but the sun appears also to revolve round the earth *in the ecliptic.* There is every possible analogy between the two motions;

both are from the west eastward, both partake in the apparent diurnal motion of the heavens westward; the only apparent difference lies in this, that the one makes more than twelve revolutions while the other makes one revolution. Now the sun does not, as we have shown, revolve round the earth as he seems to do; on the contrary, it is the earth which revolves round the sun. What shall we say, then, of the moon, whose apparent motions have so striking an analogy to those of the sun? Does the earth revolve round the moon, too, as well as the sun? In the first place we may remark, that it is impossible the earth should revolve round two centres in two different periods, both being at rest. Let us, however, consider if the analogy holds throughout. In the first place, by reason of the quicker revolution of the moon, it is a probable hypothesis that she has not so far to travel to complete that revolution as the sun (or rather the earth) has, and, therefore, that her distance is not so great. Now, by observations precisely analogous to those which we before explained for determining the distance of the sun, it is ascertained that this is really the case; the distance of the moon is only one-hundredth part of that of the sun, or 237,000 miles, and might be nearly twice included within the mass of the sun. Now knowing the distance of the moon, and knowing, by observation, her apparent diameter, that is, the angle which her real diameter makes to an observer at the earth, we know, by the solution of an isosceles triangle (of which we know the sides and vertical angle), what her real diameter is. And from this diameter we can find what is her surface, and her bulk or volume. The mean distance of the moon from the earth's centre is 60·2379 radii of the earth, and her mean apparent diameter is 31′ 7″, whence it may be calculated that her real diameter is about $\frac{3}{11}$ of the diameter of the earth, and her volume $\frac{1}{49}$. Now the apparent revolution of the moon round the earth can only be accounted for in two ways,—either by supposing this apparent motion to be a real motion, or by supposing the apparent motion of the moon round the earth *not* to be *real*, but to arise from a real motion of the earth round the moon. This last hypothesis is rendered exceedingly improbable by the exceeding smallness of the moon in comparison to the earth,—it is an exceedingly improbable supposition that the earth should revolve continually round a body only $\frac{1}{49}$ of its own bulk,—and this improbability becomes an actual impossibility when we take into account the fact which has elsewhere been proved, of the annual revolution of the earth about the sun. It is *impossible* that the earth should revolve every (lunar) month about the moon, and revolve at the same time annually with it about the sun, and yet the sun present those appearances which we know it to present; its motion in the ecliptic would at once lose all its regularity: instead of advancing continually among the signs, it would at one time move forward with a velocity of 14° daily through the sky, and at another retrograde 12° daily: in short, it is useless to enter further into the discussion,—the hypothesis is manifestly impossible; and admitting the earth to revolve round the sun in the interval of each solar year, we are compelled to admit that the moon accompanies it in this, its annual revolution, at the same time *continually revolving* round it in the period of each lunar month.

The most remarkable phenomenon in the appearance of the moon, is that series of changes which takes place during each of its synodical revolutions in the form of its disc, and which we have described as the variation of its phases. We are now in a condition to explain these completely.



So great is the distance of the sun in comparison with the radius of the moon's orbit (400 times greater), that the rays of that luminary which fall upon different parts of the orbit may be considered to be all parallel to one another. That portion of the sphere of the moon which is turned towards the rays of the sun is, in all its positions, enlightened, whilst the opposite portion or hemisphere is in darkness. Now if this hemisphere of the moon which is always enlightened, were also always turned towards the earth, the moon would always be visible; she would always appear full. This, however, is far from being the case; the enlightened hemisphere is in all the positions of the moon that turned towards the sun, and therefore, there are certain of these positions in which it is turned altogether from the earth; certain others in which it is partially turned from it; and only one position in which it is altogether turned towards the earth. Thus then, there are certain positions in which we see none of the moon's enlightened disc, *i.e.*, we do not see the moon at all, certain others in which we see only part of it, and only one position in which we see the whole disc.

The orbit of the moon is but slightly inclined (at an angle of 5°,) to the plane of the ecliptic, that is, to the plane in which the earth revolves about the sun. Hence, therefore, there are certain positions of the moon in her orbit in which she lies between us and the sun, and *certain others in* which we lie between her and the sun. Let A, A_1, A_2, A_3, represent different of those positions of the moon in her orbit, and draw planes AB, A_1B_1, A_2B_2 through her centre, perpendicular to the direction of the sun's rays falling upon her in each of these positions. Then it is manifest that AMB, $A_1M_1B_1$, $A_2M_2B_2$, &c., will be the enlightened hemispheres of the moon's surface, and that the opposite hemispheres will be dark; also, that in the position M no part of this enlightened hemisphere can be seen from the earth; that in the position M_1 only the portion B_1C_1 can be seen; in the position M_2 only B_2C_2; in the position M_3 only B_3C_3, &c. If the moon were near enough to us, we should thus see at different times portions of her surface, which would have the appearance of different sections of a spherical surface. But by reason of the great distance of the moon, we cannot distinguish that variety of shading by which the portion of the enlightened surface which we see may be ascertained to be that of a solid; and it appears to us to be perfectly flat. The width of this perfectly flat portion which we thus see is in the position M_1; the distance between two lines drawn, one from E to the boundary B_1 of the enlightened part, and the other to the boundary C_1. And this distance C_1n is what is called the versed sine of the angle $B_1O_1C_1$. Now this angle $B_1O_1C_1$ is easily seen to be equal to the angle O_1EM; that is, it is equal to the angular distance of the moon from the sun; or, as it is termed, the moon's elongation. The width of the moon's disc is, therefore, always equal to the versed sine of the elongation.

Now the versed sine of an arc increases as the arc increases up to 180°, and then diminishes in the order and degree; thus, then, the width of the apparent disc of the moon increases until her elongation is 180°, and then diminishes in the same order until it is 360°, or until she returns to conjunction with the sun again. It will be perceived that she wanes always in respect to that portion of her disc which lies toward the direction from which she is moving. Now she moves from west to east; her horns lie, therefore, always towards the west when she is waning. In this hemisphere, when we look at the sun and moon, we look always towards the south, and the east is then to the left of us; thus the moon's horns lie always towards the right when she is waning, and in the contrary direction when she is waxing or increasing. The plane which divides the enlightened from the unenlightened hemisphere of the moon, is perpendicular to the direction of the sun's rays falling upon it; that is, it is perpendicular to the plane of the ecliptic. Also the plane which divides the portion of the moon's surface which *may* be seen from the earth from that which *may* not, is perpendicular to a line drawn from the eye of the observer to the moon's centre; this plane is, therefore, variable with the position of the observer on the earth, and the position of the moon in the heavens above him: if the moon were in his zenith, this line would be exactly that joining the earth's centre and the moon's; and there will be no great error in supposing it always to have that direction. The plane dividing the part of the moon which can be seen from that which cannot, is then perpendicular to the plane of the moon's orbit; and the intersection of this plane with the plane perpendicular to the ecliptic which bounds the enlightened and unenlightened portions of the moon, is the line which joins the horns of the moon; thus this line joining the horns is nearly perpendicular to the ecliptic, deviating not more than 5° from it.

By observations which have been made on the spots on the moon, it has been ascertained that she always turns the same portion of her surface, *i.e.*, the same hemisphere, towards the earth; whence it follows that the moon must, in each revolution round the earth, turn round upon herself in a plane parallel to the plane in which she moves. This phenomenon will be understood by an illustration: if a person walk round a tree, keeping his face continually towards it, he must, to do this, when he has walked round the tree, have at the same time turned completely round upon *himself*; this will be *evident*, if we reflect that in thus moving round the tree he will have seen completely round the horizon, to effect which, he must, of course, have turned himself completely round. Instead of revolving once in every twenty-four hours about her axis, as our earth does, the moon revolves therefore once a month. Thus the lunar day is about fifteen days in length, and the lunar night fifteen days. In nineteen years there are to the inhabitants of the moon, only 235 nights and 235 days. The heat accumulated through each fifteen successive days of sunshine, and the cold produced by the radiation of this heat during 15 days of continual night, must each be excessive, and each alternation of day and night present the miniature image of a *season* of fiery heat and of intense cold. Since the moon presents always the same hemisphere to the earth, and turns away always the same hemisphere or side of her surface from it, it is evident that the earth can only be visible at any time from one half of the moon, the inhabitants of the other, if there be any, never seeing it, unless they pass the boundary which divides it from the first. To those inhabitants of the moon who can see the earth, it presents the appearance of phases like those which the moon presents to us, but on a far larger scale; first they see a crescent, thirteen times greater than that of the moon to us, then a half circle of corresponding dimensions, and, lastly, a full orb, spreading itself over a vast space in the heavens, presenting to them, as the earth turns rapidly upon its axis, in the succession of portions of its surface covered by water or broken into the inequalities of the dry land, an endless variety of appearance, and with the changes of the dense atmosphere that surrounds it, colours probably of changing hues, and light of varying intensity. This magnificent object thus goes through its series of changes apparently at rest, and fixed invariably in the same quarter of the heavens.

So slow is the revolution of the moon upon her axis, and so slow, consequently, is the progress of the boundary of light and darkness over her surface, that it would be possible, perhaps to outstrip it, and travel faster than the day. Thus completing the circuit of his planet every month, a lunarian might live in perpetual sunlight and continual heat, an expedient which the intense cold of the lunar night would not fail to suggest to him*.

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* We have stated that this expedient is possible, and the reader will readily see how. The circumference of the moon is $\frac{3}{11}$ *that of our earth, or* it is about 6790 miles, which distance might be travelled, at the rate of 226 miles a day, in about 30 days. Now the force of gravity on the moon is only about one-half that at the earth,

A POPULAR COURSE OF CHEMISTRY.

IX.

Iodine.—Bromine.—Fluorine.

Having examined the properties of oxygen, chlorine, and hydrogen, and two of their most important combinations, namely, *water*, and *muriatic acid*, we will leave pneumatic chemistry for awhile, and commence the investigation of some other curious elementary bodies, the first of which is *Iodine.*

This singular substance was discovered, in 1812, by M. Courtois, a a chemical manufacturer at Paris; but for some years after the discovery, iodine could only be procured in very small quantities: of late, however, the processes for obtaining it have been so much improved, that large quantities of iodine are now to be found in the market, at a very reasonable price.

Now, although I shall inform you how iodine is extracted from its combinations, yet I do not recommend you to undertake the process; you will save much time and expense, if you at once obtain half an ounce of iodine from the "operative chemists."

There is a substance called *kelp*, which is the alkaline ash produced by the combustion of *sea-weed;* vast quantities are annually prepared for the use of the soap manufacturer, who avails himself of the alkali it contains, to combine with oils or fatty matters, so as to form soap. M. Courtois, I believe, was engaged in the manufacture of soap, and he found that, at one particular period of the operation, the metal boilers, containing the lixivium of kelp, were invariably corroded.

Searching for the cause of this corrosion, he discovered iodine. "But for this circumstance, nearly accidental," observes Dr. Ure, "one of the most curious substances might have remained unknown, since nature has chiefly confined it to what the Roman satirist considers as the most worthless of things, the vile sea-weed."

The process for obtaining iodine from kelp is as follows:—

The kelp is put into cold water, so as to form a lixivium; this is evaporated till a pellicle appears on its surface, and then suffered to crystallize; the *mother water* (vol. i. p. 236,) is poured off, and evaporated to dryness; the dry mass is next put into a glass *alembic*, with half its weight of *sulphuric acid.* The alembic is a very useful apparatus; it was invented by the alchymists, and consists of a lower part, shaped somewhat like a Florence flask, which is called the *body*, and an upper

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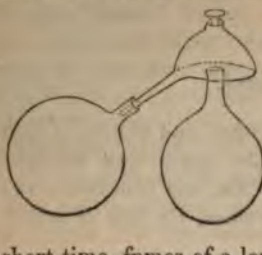
so that the resistance of friction, the effect of acclivities, and all other causes tending to retard locomotion, are only half as great on the moon's surface as here, whilst the effect of a given developement of muscular force in *causing* locomotion is twice as great. Supposing then the inhabitants of the moon, if there be any, to be provided with powers of locomotion (whatever they are,) equivalent to those which are placed at our command, it is clear that these, acting under circumstances so much more favourable, would be quite equal to a journey of 226 miles daily, and the whole population of the moon might thus be in a state of continual migration, living perpetually perhaps about the boundary line of light and darkness, so as to avoid at once the fierce heat of the direct sunlight, and the terrific cold of the mid-night.

part, fitted on by grinding, called the *head;* a small tube is united to one side of this; it is called the *beak;* and in the top of the head is an aperture, fitted with a ground-glass stopper; this is the *tubulure;* to the *beak* of the alembic is adapted a glass globe, called the *receiver.*



These various parts will be better understood from the annexed sketch.

Well, then, the body of the alembic is set upon a sand-bath, and heat gradually applied to the enclosed materials; after a short time, fumes of a lovely violet colour arise, which pass over, and are condensed in the receiver in the form of minute brilliant crystals; these are crystals of *iodine.*

The name of *iodine* was conferred on this substance from the circumstance of its producing a *violet* vapour.

Such is an outline of the process for obtaining iodine; and now let us examine its properties.

Iodine is a solid crystalline substance, of a blueish-black colour, having a strong metallic lustre; it is very heavy: drop a fragment of it into water, and you will find that it will sink in that fluid. Its specific gravity is = 4·946, water being = 1·000.

It stains the skin of a bright yellow colour, which, however, vanishes in the course of a short time; it corrodes most animal and vegetable bodies: this you will very easily find to be the case, if you put a little iodine into two phials, and tie a bit of goldbeater's skin, or bladder, over the mouth of one, and cork the other; leave them for a day or two, and both substances will become yellow, corroded and friable.

But, you may ask, How does this corrosion take place, for the iodine neither touches the bladder nor the cork? Why, this is the reason; iodine is volatile at common temperatures, and therefore its vapour soon fills both bottles, and corrodes the animal or vegetable matter with which they are closed; hence iodine should always be kept in a glass-stoppered bottle.

Take a small pane of clean glass, place on its centre a fragment of iodine; hold the pane on a level with the eye, and you will see that iodine is volatile at common temperatures, by a faint violet vapour arising from it.

If you elevate the temperature, as by holding the pane of glass above the flame of a candle, you will see the violet vapour rising abundantly; and, upon the continuance of the heat, the *iodine* will fuse, then boil rapidly, and evaporate in dense violet fumes.

Be cautious that you inhale none of these,—for iodine is a *deadly poison.*

The vapour of iodine can be exhibited in a far more striking manner on a larger scale; and I will now explain to you how to prepare and make the experiment.

Take a Florence oil flask; cut away its rushy covering, and cleanse its interior from adhering oil; this is best done by putting into it about half unce of strong nitric acid, and then gradually applying the heat of a spirit-lamp to the bottom of the flask, and by turning it round and round in various directions, so as to let the acid come into contact with every part of the flask, you will find that all traces of the oil disappear, because it is decomposed by the nitric acid; now let the flask cool, and wash it well out with water: wash it for the last time with *distilled water*, or a little strong spirits of wine, and set it to drain.

You require the flask to be perfectly dry inside; how is this to be done? It can be done by means of a bent bottle-brush, having some soft tow or cotton wrapped round it, as the butler would wipe out a decanter; but this is rather hazardous with such thin glass, and also a very tedious and imperfect operation. You can easily dry the inside of the flask as follows.

Hold it at a little distance from the fire (or hold it cautiously over the chimney of a reading-lamp); turn it round and round, so that the heat may convert the water into steam; then insert the nozzle of a pair of bellows quite into the neck of the flask, and blow out the steam; repeat the operation of warming and blowing until you see no more steam issue, and then you may be sure that the flask is perfectly dry inside.

I said just now, that the flask was to be washed out the last time with distilled water or spirits of wine, and for this reason; common water always contains more or less saline and earthy impurities, and if you simply contented yourself by washing the flask out with common water, and then warming and blowing air in, you would have the saline or earthy matters of the water left behind, soiling the interior of the flask; whereas, by using distilled water, or spirits of wine, both of which, if pure, are wholly volatile, the flask remains bright on its interior.

If the nozzle of the bellows will not enter the flask as directed, you can easily lengthen it by a bit of pewter or copper gas-tubing, temporarily adapted to it.

This is the way in which the chemist dries the interiors of flasks, retorts, alembics, receivers, and other glass vessels, of far more complicated shapes.

The Florence flask being dry and cool, put two or three large fragments of iodine into it, and hold a spirit-lamp so that the flame plays gently on the part containing the iodine; you will soon find a copious evolution of violet-coloured fumes arising, and, as the heat is continued, they become darker and darker, until at length you can no longer see through the thick atmosphere of intensely violet vapour.

This vapour is very heavy, as may be shown by holding the mouth of the flask over the edge of a glass tumbler, or tall ale glass, that has been previously made very hot by standing before the fire; you will find that you can actually *pour* the vapour out of the flask into the glass, where it will remain until the glass becomes cool, and then it condenses into brilliant crystals. The use of warming the glass is, of course, that it may not chill or condense the hot vapour.

You will find also the upper part of the flask studded with crystals, and this process, as well as that already spoken of for obtaining iodine, are good illustrations of what chemists call sublimation, (vol. i. p. 237,) namely, the conversion of a solid substance into vapour by the application of heat, and the subsequent condensation of the vapour by cold.

If you take another dry flask, put some iodine into it, and sublime it at a very gentle heat, you will find that its vapour condenses in crystals of great size and beauty. So also if you put some at the bottom of a large glass-stoppered bottle, and place this on the warm chimney-piece for a day or two; generally speaking, the slower the sublimation the larger will the crystals be.

Sublimation is a very old alchemical process, but one that is followed constantly at the present day, not only in the private laboratory, or the public lecture-room, but also on an enormous scale in some chemical manufactories. All those substances so well known to you under the names of flowers of sulphur, sal ammoniac, camphor, volatile salt, corrosive sublimate, calomel, and many others, are produced or purified by the process of sublimation.

Iodine is very sparingly soluble in water; the small fragment that you dropped into water with the view of proving that it was denser than that fluid, will very likely not all dissolve, but part will, and form an *aqueous solution* of iodine, having a brownish-yellow colour.

The solubility of iodine in water is variously stated by different chemists; but I think that we shall not be far wrong in stating that a pint of water will dissolve only one grain of iodine.

If however, instead of water, you employ spirits of wine (alcohol) as the solvent of iodine, you will find that it dissolves a very large quantity; pour some spirit of wine into the flask used for sublimation, and still containing some iodine; a very deep brownish-yellow solution of iodine will be produced.

If you add yet more iodine, of course a much stronger solution will be obtained; make one approaching to saturation, and then pour some of it into water, a large quantity of iodine will instantly precipitate in the solid state, because the water having a strong attraction for the spirit of wine, takes it away from the iodine, which is thus compelled to precipitate, as it is not soluble to any very great extent in the resulting mixture of water and spirit. You will, of course, directly infer from this, that the stronger the spirit the stronger will be the alcoholic solution of iodine.

Iodine supports the combustion of some substances; thus, potassium will burn in its vapour, but the experiment is rather difficult to manage nicely; you can, however, easily convince yourself that iodine possesses some claims to the rank of a "supporter of combustion," by putting a little of it into a saucer, and dropping on it a bit of *phosphorus* slightly damp; in the course of a second or two, the two substances manifest a strong affinity for each other, and they combine, with the evolution of flame, producing a mixture of the two *iodides of phosphorus*, which remains in the saucer as a compound of a reddish colour.

The violet colour of iodine is in most instances a sufficient test of its presence, but a far more delicate test is the action of *starch* upon a solution of iodine, with which it immediately produces a precipitate of a fine dark-*blue* colour.

Take a tea-spoonful of "arrow-root," which is a very pure variety of starch; put it into a wine glass with a tea-spoonful of cold water, and mix it well; then fill up the glass with *boiling water*, stirring the while, and you get a clear solution of starch, which, if too thick to pour readily, may be diluted with water to a thinner consistency.

Pour some of this solution into a glass, and add to it a few drops of the aqueous solution of iodine; a dark-blue precipitate will instantly appear, although, as I have already said, the quantity of iodine present is only about one grain in a pint.

Starch, therefore, is a test of iodine. You can try the experiment with solutions of various strengths, and thus gain a notion of the delicacy of the test.

But starch is only a test of iodine when the latter is in *a free* or *uncombined state*, as it exists, for example, in the solid form, or in the aqueous or alcoholic solutions; for if iodine be combined with *a base*, as, for instance, with *potassium*, forming *iodide of potassium*, then starch fails to detect its presence.

Obtain a few crystals of *iodide of potassium* (or *hydriodate of potassa* is the name by which it is better known at the shops); make a solution of it in water, and test for iodine with the starch; you will get no precipitate, and yet in every 165 parts of the *iodide* there are 125 parts of iodine, but being combined with 40 parts of potassium, the starch cannot detect it.

The iodide of potassium is very easily decomposed, by many agents; *chlorine*, for example, will instantly decompose it, and liberate the iodine in a free state.

Take a very small bottle of chlorine, and pour it (vol. ii. p. 295) into the glass containing the mixture of iodide and starch; and the instant that the heavy chlorine descends to the surface of the mixture, the iodide is decomposed; the iodine is liberated in a *free* state, and meeting with the starch, the deep-blue and highly characteristic precipitate is produced.

The solution of iodide of potassium may be decomposed by voltaic electricity; take the small glass, with platinum wires, as shown at p. 21 of this volume, fill it with the solution of the iodide mixed with starch; then charge the battery, and connect its opposite ends with the platinum wires, (as for the decomposition of water,) and the moment that this is done, iodine will be evolved in the free state at the *anode*, where meeting with the starch, the *blue* colour is produced. Like oxygen and chlorine, therefore, iodine is an *anion*.

The principal use hitherto made of iodine is in medicine, and its alcoholic solution is then employed; it has been found efficacious in many glandular complaints; with these matters, however, we have nothing to do, and therefore I quit them with only a passing notice.

Iodine unites with oxygen, chlorine, and hydrogen, under certain circumstances, producing *iodous*, *iodic*, and *periodic acids*, *chloriodic acid*, and *hydriodic acid;* these compounds have very little popular interest, but some of their combinations with bases are important, as will be shown at a future opportunity.

Iodine unites with many of the metals, with *potassium*, for instance, as already mentioned. When united to *mercury*, it forms an *iodide of mercury*, which is a compound of a most brilliant scarlet colour, of some use in painting, although not a very permanent colour; it is known to artists by the absurd name of *essence of vermilion!* Vermilion is a *sulphuret* of mercury. Iodine is a simple or elementary substance; its compound nature is strongly suspected, but hitherto chemists have been unable to decompose it, and therefore it ranks with other elements; its equivalent or combining proportion is = 125.

BROMINE.

This singular substance was discovered by Balard in 1826, and is obtained by a circuitous and difficult process from *bittern*, which is the uncrystallizable residue of sea-water.

I do not enter into the details of the process, because they are not sufficiently simple for these popular essays; neither do I advise the young chemist to meddle at all with this very dangerous and deadly poisonous substance. I shall merely state a few of its properties. Bromine is a liquid of a deep reddish-brown colour, very volatile, and having a most suffocating and disagreeable odour; hence its name, from βρωμος. It dissolves in alcohol and æther, destroys vegetable colours; it does not support common combustion, but phosphorus, and some of the metals, burn it vividly.

It is so virulently poisonous, that a single drop placed upon the beak of a bird caused instant death. Bromine cannot as yet be resolved into any simpler forms of matter, and therefore chemists reluctantly place it amongst the already numerous class of elements. Its combining or equivalent property is 78; it combines with chlorine and iodine, forming *chloride* and *iodide* of *bromine;* with many metals it also unites, forming *bromides.* Some of these are perhaps important: the *bromide of potassium* has been lately introduced into medicine.

FLUORINE.

I cannot tell you much about *fluorine*, for it has never been obtained in a free or insulated state; and it is only ranked with oxygen, chlorine, iodine, and bromine, from analogy.

The researches of the late Sir H. Davy tended to prove the existence of a peculiar substance in *fluor spar*, and he proposed for it the name of *fluorine*, but the utmost exertions of his refined chemical skill failed in eliciting it in a pure state; he found its powers of combination so intense, that no substance was capable of effectually resisting its energetic action, and he remarks that *fluorine* almost realizes the "alkahest," or universal solvent of the alchemists. Two foreign chemists, Baudrimont and Pelouze, have lately announced, that they have succeeded in obtaining fluorine in an insulated state; they describe it as a gaseous body of a deep brownish-yellow colour, having an odour similar to that of chlorine.

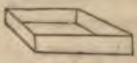
They also say that it is a comparatively *inert body*, which is quite incompatible with all our previously received notions of its nature. I am not aware that their experiments have been repeated in this country. An account of these will be found at p. 250, vol. ii. of this Magazine.

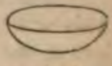
I wish now to direct your attention to a very singular acid, which may be elicited from *fluor spar*, and which would appear to consist of *fluorine and hydrogen*, and therefore called *hydrofluoric acid.*

This acid was discovered by the celebrated Scheele; but the process for obtaining it cannot be undertaken without an expensive apparatus constructed of pure silver; therefore the only experiment that I shall adduce concerning it will be its singular action on glass.

Take a piece of lead pipe, such as is used for water-pipe, about an inch in the bore, and six or seven inches long; beat up one end of it so that it will hold water, and when you find that it will do so, empty out the water, and wipe out the tube perfectly dry with a soft cloth. Now reduce a lump of *fluor spar* to fine powder, and put about half an ounce of this into the leaden tube or bottle, as it may be called, and pour on the powder about twice its weight of strong *sulphuric acid*. Hold the tube with a pair of tongs, or with a wire twisted round it so as to form a convenient handle, over the flame of a spirit-lamp; let the flame play upon its closed end; you will soon find abundance of vapours given off, which are those of the *hydrofluoric acid:* and its action on glass may be shown by holding a glass rod, or a thin strip of window glass, in the aperture of the tube, so that the vapours may envelope it; the polish and transparency of the glass will be speedily destroyed; it will soon look like *ground glass*, and if you continue the experiment for some time longer, it will be very deeply corroded and eaten away. The vapour of hydrofluoric acid is very pungent and irritating, therefore you must take care to avoid its inhalation; and now, having made this preliminary experiment, another can be undertaken to show the method of *etching upon glass.*

Make a small tray or box out of a piece of sheet-lead, about four inches square, and one inch and a half deep, which you can easily do, by cutting slits in the lead, and then folding it up into a square form, just as if you were going to make a box of card-board; or you may take a piece of sheet-lead, and press it into the hollow of a basin so as to mould it into a concave shape, like a shallow cup, and then trim off the edges flat and smooth with a pair of scissors; either of these shapes will do. Place the leaden vessel on the ring of a retort-stand, and put in some powdered fluor spar and sulphuric acid, as before directed. Lay a plate of glass so as to form a lid to the vessel, and proceed to heat the mixture gently; when, as the *hydrofluoric* acid rises, it will act upon and corrode the glass.





But now, supposing that you take another plate of glass, and warm it before the fire or upon the hob of the grate until it is very hot, and then rub a bit of bees'-wax over its surface, and allow it to cool; you have thus coated the glass with wax, and this will resist the attack of the acid to a very great extent; now, with a needle point, or a brad-awl point, trace out, quite through the wax, any design you choose,—initials, words, flowers, &c. Place the pane of glass, with the waxed surface downwards, over the leaden vessel, which is slowly evolving *hydrofluoric acid*. This will act upon and corrode the parts of the glass that are free from the wax; and after the lapse of about a quarter of an hour, you may stop the operation; melt off the wax before the fire, or wash it off with spirits of turpentine, when the design that you traced will now be found deeply and unalterably *etched* into the glass.

You may not succeed, perhaps, in your first trials, but a very little practice will soon ensure success. It must be obvious that if you wish to etch a copy of any drawing or print in outline, that you must lay the waxed pane on its surface, keeping it steady there by weights, and then you can trace the outline on the wax; but be sure that the point of the tracing-needle perfectly removes the wax from the glass, or otherwise, when it is placed over the *hydrofluoric* acid, some faulty lines will appear. Many chemists thus etch labels on bottles, and it is a very good plan, especially with such as are destined to hold strong acids, which soon destroy written or printed labels. The reason why you can thus etch or corrode glass is, on account of the strong attraction which hydrofluoric acid has for *silica*, one of the constituents of glass, with which it forms *fluosilicic acid.*

I have recommended the use of leaden vessels for this operation, because lead is the cheapest metal upon which the acid exerts but little action; but vessels of silver are employed when it is an object to obtain pure *hydrofluoric* acid in the *liquid* state; and this is done by distilling the mixture already spoken of in silver vessels, and receiving and condensing the vapour in vessels of the same metal.

When thus obtained, it is a limpid colourless liquid, very volatile, and having so great an attraction for water, that when dropped into it a hissing noise is produced; it has a most energetic action upon animal and vegetable substances, and if accidentally dropped upon the hand, instantly produces a most painful sore, very difficult indeed to heal,—so that extreme caution is necessary in handling the vessels containing it. When diluted with water, it is employed for etching on glass, simply by pouring it on the waxed pane.

The composition of the *hydrofluoric acid* has not been satisfactorily made out, but it probably consists of *hydrogen* 1 + *fluorine* 18 = 19, and *fluor spar* is regarded as a *fluoride of calcium.*

The next elementary body that I shall bring before you is *nitrogen.* This will bring us back to pneumatic chemistry, and will form the subject of the next paper.

MISCELLANEA.

Accidental Production of Animal Life.—Mr. Crosse.

Among the great number of persons in the present day, who, without being professors or *savants*, take a lively interest in the novelties which modern science is incessantly pouring out, we think we may fairly presume that very few are now ignorant of the name of Mr. Crosse. His meteoric appearance at the Bristol session of the British Association, was of a nature to excite curiosity, far and near. The simplicity, the earnestness, the candour, and the equanimity, with which this scientific recluse described his protracted watchings, his hopes, his failures, and his successes, will be long, very long, remembered by all who heard him. Unconscious, on joining the Association, that he possessed anything to communicate which could interest such an assembly; unambitious, when assured to the contrary, of the flattering distinction which such communications must confer; and when constrained by scientific friends to present himself to the public eye, he was perfectly unawed by the fixed intensity with which it regarded him.

As might naturally be expected, and probably by no one more than by Mr. Crosse, several of the experiments and the results communicated by him, were already known to many of his auditors. Looking at their position in the scientific universe, it would have been matter of little surprise, if every one of the facts which he had solitarily arrived at, had been long anticipated by one or other of the philosophers whom he addressed. But, after examination and re-examination, quite sufficient appeared to have remained, not only to excite the admiration, and gratify, in a most unexpected degree, his fellow-labourers in the investigation of nature, but to inflame the jealousy, and awaken the envy, of some whose acquisitions in the field of science might have satisfied, we should have thought, any ordinary lust of fame. The usual mischievous consequences followed, "faint praise' doubt, misrepresentation, and finally, denial of all novelty, merit, or even honest intention, succeeded each other in the accustomed order. He who was undazzled by the glory which geologists so unsparingly threw around him, was little irritated or annoyed by the gall of certain *physiciens;* but his friends, and the advocates for fair play, did not permit the series of attacks to pass on uninterrupted. One of them, after enumerating the more aggavated, inquires, "But what are the facts, and how bears the truth? Why that Mr. Crosse, and other patient investigators at home and abroad, had been simultaneously and independently carrying out, and verifying by experiment, the principles of the fathers of this section of science. Modifications, however, in the mechanical part of the apparatus, such as varnished boards instead of metal plates, and water instead of acids, had been long ago used before we were told of the 'bucket of water and the brick-bat:' artificial crystallization, the result of the action of electro-magnetism on a menstruum containing the simple earths or metals in solution, had been proved, particularly by French chemists, years before the Bristol meeting.

"Becquerel, especially by means of very weak galvanic tension, had obtained perfectly-crystallized forms of the oxides and sulphurets of various metals. M. Aimé had produced crystals of specular iron similar to those from Elba, and also crystals of *pure iron,* unknown before to mineralogists. The researches of Mr. Fox in our own country, and M. Fournet abroad, had long ago almost proved that electro-magnetism was the principal agent in determining the aggregation of metals from volatile products in mineral veins; but if Mr. Crosse had these and other unquestioned and well-recorded predecessors in the proud career of discovery in this branch of science, has he therefore effected nothing? He has done more with his 'bucket of water and brick-bat' in the quiet of our own Quantocks, than Dr. Ritchie has ever effected with the full command of one of the most powerful voltaic batteries in Europe—the instrument which enabled Sir H. Davy to make the grandest discoveries of the age. Mr. Crosse was the first who produced perfectly-defined *quartz* crystals, a substance which had resisted every previous attempt in the laboratory to restore it (after analysis) to any true synthetical arrangement; and this, supposing it to be the only one, is as successful and palmy a discovery as the formation of crystals of pure iron by M. Aimé. This is his just and fair meed of fame, but Mr. Crosse will never be regarded as the source of that beautiful ray of light which first directed philosophers to search for the cause of crystallization in the agency of electro-magnetism."

Scarcely had this effervescence subsided when, in the closing month of the past year, Mr. Crosse's name was sent up again for the gaze of the scientific multitude, and like a rocket of superior calibre and longer pole, rose to a still more extraordinary height. It was announced, that, pursuing the same direction of inquiry, with the same powerful apparatus, and the same unremitting application, he had produced organic arrangement and excited the vital principle in inorganic matter. The scoffers received this announcement with increased unbelief but unmingled gratification, and were convulsed with their own facetiousness; at the same time, it must be admitted that the most ardent of the admirers of Mr. Crosse, who were not visiters of Broomfield, or in direct communication with him, were disposed to think this report a malicious exaggeration, and though it acquired new force and more extensive currency on each succeeding day, yet they perceived it with regret, and dreaded to hear from any authentic source that it has proceeded from Mr. Crosse himself. Early in January last, the following letter from that gentleman appeared in the *Taunton Courier*.

Broomfield, Jan. 3, 1837.

Dear Sir—Having seen in a very recent publication what is stated to be an account of some experiments of mine, in which insects were produced instead of crystals, I take the *earliest opportunity of making known*, that such an account was published without my knowledge, and that, although the main fact is as there represented, yet the mode of conducting such experiments is inaccurate. I am the more anxious to correct this statement, as several *perfectly* erroneous accounts have already appeared in different papers, calculated to produce a false impression in the minds of all but scientific men, who can, if they please, readily distinguish an error when they meet with one.

I am sorry to observe that a gentleman of high repute, Dr. Ritchie, who seems to have forgotten both science and temper on the present occasion, has taken advantage of some such misstatements to form the weak foundation for a most illiberal attack upon me in the *Literary Gazette*, which I shall take an early opportunity of replying to as it deserves.

I must further request my friends and the public not to give credit to any publication as coming from me unless my name is attached; and as I detest nothing so much as a literary or scientific dispute, I had hoped to have glided through the remainder of my life without provoking the malevolence of the ill-disposed, and more particularly as I am unconscious of having done anything to offend the most captious by presumption or misrepresentation.

I beg, &c.

A. Crosse.

The scorners were not in the least disconcerted by this communication. "*Taunt on*," was their exulting cry, the laugh grew louder, the fun more furious, and the clamour was as triumphant as ever, when a communication from Mr. Stutchbury, of the Bristol Institution, was disseminated through the public journals, of which we give a copy. Though it is not authenticated by the signature of Mr. Crosse, and therefore has not the test by which Mr. C. requested the public, in his letter of the 3rd of Jan., to decide upon the merits of any publication coming from him, yet as there is no doubt of the honour and veracity of Mr. Stutchbury, the extract which this gentleman gives from a letter of Mr. Crosse, may be considered as if communicated by Mr. Crosse himself.

Sir—It may be truly said that facts recorded, faithfully detailed, and made public, are the means by which Philosophy is enabled to render her temple more durable on its foundations—every additional fact being the commencement of that which, when understood, forms an outwork of defence, rendering the interior the strong-hold and sacred depository of truth.

It was a maxim of the late John Hunter, which he was repeatedly proclaiming, that greater benefit would be conferred upon the community if professional men had the moral courage to publish detailed accounts of all their unsuccessful cases, than could be derived by the publication of those which met with a favourable termination, for then the physician would reject at once such as had failed as modes of cure, without repeating experiment after experiment, thereby saving much valuable time, and certainly sparing much needless pain to the unfortunate patient. So with facts in natural philosophy. Let experiments be recorded, and their results will enable others, either to avoid, or successfully to pursue them, so as to bring them to bear upon principles which may elucidate some great truth, the light of which would probably have never shone upon them, had they not been able to take advantage of the works of pioneers who had proceeded, perhaps, to the very threshold of the same result.

With regard to a large number of curious and (at the same time, in consequence of the principle not being understood,) astounding facts, the investigator would be doing a great injury to the cause in which he labours, should he be deterred from making them known, merely because he cannot at the same time reasonably account for the same. And upon this view of the subject, and to set at rest the vague views which are abroad, I feel it a duty due to the cause in which I am engaged, and to the philosopher with whom they have originated, at once to take the liberty of transmitting to the press, and particularly the local press, from which so many reports have emanated, an abstract of a letter I have received from Mr. Crosse, with an account of his experiments, in the language of a private communication (not that which he would probably have chosen, had he made the communication himself), and without further comment.

"The following is an accurate account of the experiments in which insects made their appearance:—

"*Experiment the first.*—I took a dilute solution of silicate of potash, supersaturated with muriatic acid, and poured it into a quart basin, resting on a piece of mahogany; a *Wedgwood* funnel was placed in such a manner that a strip of flannel, wetted with the same, and acting as a siphon, conveyed the fluid, drop by drop, through the funnel upon a piece of somewhat porous Vesuvian red oxide of iron, which was thus kept constantly wetted by the solution, and across the surface of which (by means of two platina wires connected with the opposite poles of a voltaic battery, consisting of nineteen pair of five-inch plates in cells filled with water and $\frac{1}{10}$ muriatic acid,) a constant electric current was passed. This was for the purpose of procuring crystals of silex. At the end of fourteen days I observed two or three very minute specks on the surface of the stone, white, and somewhat elevated. On the eighteenth day, fine filaments projected from each of these specks, or nipples, and the whole figure was increased in size. On the twenty-second day, each of these figures assumed a more definite form, still enlarging. On the twenty-sixth day, each assumed the form of a perfect insect, standing upright on four or five bristles which formed its tail. On the twenty-eighth day, each insect moved its legs, and in a day or two afterwards detached itself from the stone and moved at will. It so happened that the apparatus was placed fronting the south, but the window opposite was covered with a blind, as I found these little animals much disturbed when a ray of light fell on them; for out of about fifty which made their appearance at once, at least forty-five took up their habitation on the shaded side of the stone. I ought to have added, that when all the fluid, or nearly so, was drawn out of the basin, it was caught in a glass bottle, placed under a glass funnel which supported the stone, and was then returned into the basin without moving the stone. The whole was placed on a light frame made for the purpose. These insects have been seen by many of my friends, and appear, when magnified, very much like cheese-mites, but from twice to eight times the size, some with six legs, others with eight. They are covered with long bristles, and those at the tail, when highly magnified, are spiny. After they have been born some time they become amphibious, and I have seen them crawl about on a dry surface.

"*Experiment the second.*—I took a saturated solution of silicate of potash,

TABLE OF THE DAILY MEAN-TEMPERATURE.

Date.	No. made of Observations.	Daily Mean-Temperatures.	Weather.
1833, June 6	7	83·8	N.W. Stormy. 5 P.M., *Tufán*[*] from N.W.
—— 7	9	81·8	N.W.
—— 8	10	84·6	N.W.
—— 9	13	84·2	N.W.
—— 10	13	89·8	N.W.
—— 11	12	83·0	N.W.
—— 12	15	84·0	N.W.
—— 13	15	94·0	W. 7 P.M. Rain-storm.
—— 14	13	86·8	W.
—— 15	12	86·2	W.
—— 16	11	84·8	W.
—— 17	12	85·2	
—— 18	14	87·0	
—— 19	9	86·3	
—— 20	13	79·7	N.W. 3 P.M., *Tufán.*
—— 21	11	87·6	N.W. A.M. 7 P.M. S.W.
—— 22	14	84·2	
—— 23	11	85·0	S.
—— 24	10	88·0	S.
—— 25	12	91·4	S.
—— 26	12	90·9	S. X. A.M. 3 P.M., Rain W.
—— 27	13	88·8	S. X. S.E.
—— 28	3	85·5	
—— 29	8	83·1	
—— 30	9	88·1	var. Showers. River rising †.
July 1	10	84·3	N.W. 4 P.M. Rainy. *Tufán.*
—— 2	12	85·7	‡
—— 3	9	85·6	N.W.
—— 4	6	85·0	W. 7 P.M. *Tufán.* W.
—— 5	8	84·4	N. 3 P.M. *Tufán.* N.
No. of days 30	Total No. made of Obs. 331	86·0	Mean of all the Observations made.

Whole period	Mean temperature		85·80
	Maximum do.	(2 P.M. June 25)	99·25
	Minimum do.	(2 P.M. June 12)	80·25
Daily mean	Maximum do.	(June 13)	84·90
	Minimum do.	(June 20)	80·00
Hourly mean	Maximum do.	(11 P.M.)	90·00
	Minimum do.	(6 A.M.)	84·30

It results from these observations, that the mean temperature of the whole period in a tent so prepared, appears to be less than that of the interior of the *Bungalows*§, which, in Cawnpore, is estimated at 88°. But the changes in the tent are far more sudden and perceptible, from the thinness of the walls.

It may assist in the estimation of this trying house-temperature, to remark that the highest monthly mean temperature, *out of doors*, in England, is, according to Prof. Daniell, but 61°·6.

We cannot help regretting, that in addition to the above valuable observations, Captain Burt did not make others, simultaneously, with thermometers placed, externally, in the shade and in the sun; though, probably, if the thought had occurred to him, a sufficient number of instruments might not have been at hand in those remote regions; with such observations, the amount of the effect of the *tatties*, &c., might have been accurately ascertained.

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[*] *Tufán.* A sudden storm of wind and dust, generally accompanied by rain, prevalent in several parts of India, a little before the setting in of the rains.

† *River rising.* The Jumna (similar to most Indian rivers,) rises for some time previous to the commencement of the rainy season. The rise is slow at first, but is gradually accelerated until it becomes rapid. I have known it to rise as much as eighteen feet in one night, apparently from the melting of the snow in the upper regions.

‡ *July* 12. At 5 P.M. the moon was seen, eclipsed apparently about three-eighths. At 5 30 P.M., she set in a cloud.

§ *Bungalow.* A house built with bricks, baked or unbaked, and generally having the roof thatched.

Scarcity of Pine Timber in the New England States.

"THE States of Maine and Massachusetts are no longer the centre of *lumber*-speculations. The yellow and white pine of commerce is now so rare in the settled parts of the United States, that their speculating lumber-men have for the three last years been *purchasing largely* of the timbered lands of Canada, as reserves for their yellow pine-timber. This is one of the causes of the unsettled differences of the boundary-line between the New England States and Canada,—the Americans being greatly desirous to get possession of the virgin forests of Canada for the pine-timber therein. Deals and boards are now prepared on the north side of the St. Lawrence, and timber is cut on the Canada side of Lake Erie, and sent along the western canal for the New York market!"—CORR.

Improved Refracting Telescope.

PROF. STEINHEIL, of Munich, has carried into effect some principles, which are spoken of as likely to produce great improvements in the construction of Refracting Telescopes of large aperture.

He lately exhibited to the Academy of Sciences at Munich an instrument of his construction, and compared it with one of the late celebrated Fraunhofer.

Focal length of the new instrument, 28 inches*; aperture, 37 lines.—Ditto of Fraunhofer's, 42 inches; aperture, 34 lines.

The first was found to equal, if not surpass, the second, though the field of view was more limited, as had been anticipated from the theory of its construction. The image it gave under a power of about 100, was perfectly achromatic and well defined. Ω

Munich, Dec. 1836.

Spring at the Summit of a Mountain.

M. DURIEU, who lately made a scientific journey in the kingdom of the Asturias, mentions the following important fact in physical geography:—"A beautiful spring flows from the highest point of the peak of the *Sarrantina;* but, as this peak is not commanded by any neighbouring summit, we must necessarily suppose that the other branch of the siphon must be placed at a great distance to the east, to receive, on the flanks of the mountains covered by perpetual snow, and having a much greater elevation, the water which issues from the extremity of the shorter branch, and which an extraordinary accident, or concealed natural cause, has forced to ascend to the top of a pointed peak."

Bronze Coinage proposed in France.

"THE question of a new coinage in France, in lieu of the present one of copper, billon, &c., is under consideration. Nothing can be much worse than the existing one; for, besides its low pretensions in relation to art, it is of all sizes, weights, designs, and alloys. The new one is proposed to be of bronze, in harmony with the decimal system of weights and measures, less heavy and embarrassing, not easily falsified, and executed in the highest style of excellence. The latter condition will very much increase the difficulty of counterfeiting it."—*Annuaire,* 1837.

Remarkable Phenomena in solar total, and annular, Eclipses.

MR. BAILY, (the present President of the Astronomical Society,) having read of certain singular appearances that are recorded as having taken place in annular eclipses of the sun, at the moment that the whole disc of the moon enters on the disc of the sun, he was desirous of witnessing those phenomena at the solar eclipse of May 15th last; and finding that the central path of the moon's shadow would pass nearly in a straight line from Ayr, on the western coast of Scotland, to Alnwick on the eastern coast of Northumberland, he proceeded to Scotland for the express purpose. Having computed, from the elements given in the *Nautical Almanac,* that the central line of the moon's umbra would pass directly over, or very near to, Jedburgh in Roxburghshire; and having ascertained that this place was within eight or ten miles of Makerston, the seat of Lieut-General Sir Thomas Macdougal Brisbane, Bart., who has a well-furnished observatory there, and from whom he was sure of obtaining the correct time for his chronometers, he resolved to make that town his head-quarters. Mr. Baily took with him a 3½ feet refracting telescope by Dollond, 2⅝ inches aperture, and magnifying about forty times; a twenty-inch Rochon's prismatic telescope, for measuring the distances between the borders of the sun and moon; two thermometers; a burning-glass; and four pocket chronometers.

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* It is presumed that this inch is the old Paris one, which is still partially used by German mathematical instrument makers; explicitness in descriptions of this kind is far more desirable than is generally imagined.

Mr. Baily took up his station at the house of Mr. Veitch, a very ingenious gentleman, residing at Inch Bonney, about half-a-mile to the southward of the town of Jedburgh, who afforded him every facility for making the observations. The morning of the 15th of May is described as being remarkably fine and clear: not a cloud to be seen in any part of the heavens during the whole time of the eclipse. The times of the beginning and ending of the eclipse, and of the formation and dissolution of the annulus, have already been given in the preceding volume of these monthly abstracts, page 200. But Mr. Baily does not lay much stress on this part of his observations—more especially those connected with the annulus—since his attention was taken up with other more interesting phenomena. He says he was in expectation of meeting with something extraordinary at the formation of the annulus; but imagined that it would be only momentary, and consequently that it would not interrupt the noting of the time of its occurrence. In this, however, he was deceived, as the following facts will show. For, when the cusps of the sun were about 40° asunder, a row of lucid points, like a string of beads, irregular in size, and distance from each other, *suddenly* formed round that part of the circumference of the moon that was about to enter on the sun's disc. This he intended to note as the correct time of the formation of the annulus, expecting every moment to see the thread of light completed round the moon; and attributing this serrated appearance of the moon's limb (as others had done before him) to the lunar mountains; although the remaining portion of the moon's circumference was perfectly smooth and circular, as seen through his telescope. He was somewhat surprised, however, to find that these luminous points, as well as the dark intervening spaces, increased in magnitude: some of the contiguous ones appearing to run into each other like drops of water. Finally, as the moon pursued her course, these dark intervening spaces were stretched out into long, black, thick, parallel lines, joining the limbs of the sun and moon; when, all at once, they *suddenly* gave way, and left the circumferences of the sun and moon in those points, as in all the rest, apparently smooth and circular, and the moon perceptibly advanced on the face of the sun. This moment of time Mr. Baily considers to be that which most persons would assume and record as the formation of the annulus; but he adduces strong reasons afterwards to show that the true formation of the annulus was some seconds prior to that event.

After the formation of the annulus, as thus described, the moon preserved her circular outline during its progress across the sun's disc, till her opposite limb again approached the border of the sun, and the annulus was about to be dissolved. When, all at once (the limb of the moon being at some distance from the edge of the sun), a number of long, black, thick, parallel lines, exactly similar in appearance to the former ones above mentioned, *suddenly darted forward*, and joined the two limbs as before: and the same phenomena were repeated, but in an inverse order. For, as those dark lines got shorter, the intervening bright parts assumed a more circular shape, and at length terminated in a fine, curved line of bright beads (as at the commencement), till they ultimately vanished, and the annulus consequently became wholly dissolved. This remarkable and singular phenomenon was also observed by Mr. Veitch, and also by Sir Thomas Brisbane, as well as by Mr. Henderson at Edinburgh; with some slight differences, however, in the detail. The appearance of the dark lines, or threads, was likewise noticed by Mr. Bell, at Alnwick, who sent an account of the same to the Philosophical and Literary Society at Newcastle. Mr. Baily describes them to have been as plain, as distinct, and as well-defined, as the open fingers of the human hand held up to the light: and that there could not have been any doubt as to their form and existence, since they were seen by different observers, at different places, and with different telescopes. Several drawings accompanied the paper, showing the appearances at various stages of the annulus.

The number of these dark lines, or threads, Mr. Baily considers to have been about eight: in which opinion he was confirmed by Mr. Veitch. Sir Thomas Brisbane, however, thinks there were not more than six: whilst Mr. Bell, who noticed four at the dissolution of the annulus, says that there were only two at its formation. On these and other points, Mr. Baily thinks there is ample room for a diversity of opinion, since the observer is taken, as it were, by surprise, and the phenomenon itself, during the short period of its existence, is constantly varying in some minute particulars.

Mr. Baily remarks, that the diminution of light was not so great during the existence of the annulus as was generally expected, being little more than might be caused by a temporary cloud passing over the sun; the light, however, was of a peculiar kind, somewhat resembling that produced by the sun shining through a morning mist. The thermometer in the shade fell only about three or four degrees. The birds in the hedges were in full song during the whole time of the eclipse. About twenty minutes before the formation of the annulus, Venus was seen with the naked eye; and a few minutes afterwards it was impossible to fire gunpowder, with the concentrated rays of the sun, through a lens of three inches in diameter. The same lens, likewise, had no effect on the ball of a thermometer during the existence of the annulus.

For the cause of the remarkable optical deception above described, Mr. Baily does not attempt to account; but he confesses his surprise that the phenomenon has not (with one single exception, which will be presently alluded to,) been noticed, or recorded, on former occasions, since it must have been seen by every person who watched for the formation and dissolution of the annulus; and although detached portions of the phenomenon have been recorded by different observers, as seen at different places (various extracts from whose accounts are quoted by Mr. Baily), yet it is impossible from those descriptions to form an accurate idea of the whole, or to trace the origin, progress, and termination of this phenomenon, which is certainly one of the most remarkable in astronomy. M. Van Swinden is the only person who has placed on record the observation of the dark lines, or threads, which connect the borders of the sun and moon, at the formation and dissolution of the annulus. His account is inserted in the first volume of the *Memoirs* of this Society (page 146), accompanied with drawings, which coincide almost exactly with those given by Mr. Baily. In nearly all the accounts by other observers, the description of the phenomenon is restricted to the very commencement of the annulus, or to the formation of the string of luminous points which on a sudden are seen to surround that portion of the moon's limb about to enter on the sun's disc; and no notice whatever is taken of the continuation of the phenomenon, or of the stretching out of the dark spaces into parallel lines, as above mentioned: nor of their *sudden* rupture and *disappearance*, which is by far the most remarkable part of the phenomenon.

How far any of these appearances may favour the hypothesis of a lunar atmosphere, or whether, indeed, they could be accounted for on such an assumption, the author does not stop to discuss; but, with a view to assist those who are disposed to enter on such an inquiry, he has adduced various accounts of a similar phenomenon to that of the dark lines, observed at the transits of Venus over the sun in 1761 and 1769. For, on each of those occasions, many astronomers remarked that, at the interior contact of Venus with the sun (both on its ingress and egress), there was formed a sort of dark ligament between the border of Venus and the border of the sun, which appeared like a protuberance from the planet, and which continued several seconds. This dark ligament is represented, in the drawings which accompany the several memoirs on this subject, to be much thicker, and to continue longer, than the dark lines in a solar eclipse; so that the planet, during the progress of the ingress and egress, assumes a shape which has been variously described as resembling a pear, a Florence flask, and a skittle. But all the accounts agree in stating the *sudden* rupture of the ligament; and that immediately thereon the planet assumes its usual circular shape. Nothing of this kind, however, has been noticed at the transits of Mercury over the sun: on the contrary, we have the direct evidence of Sir William Herschel (who examined Mercury, with that special object in view, at the transit of November 9, 1802), that he could not discern anything out of the usual course. He expressly states, that the whole disc of Mercury was as sharply defined as possible; and that there was no kind of distortion of the limb, either at its ingress or egress: the appearance of the planet remained well defined from first to last.

Mr. Baily considers, and adduces certain facts to show, that the circular edge of the moon is always distorted at those points which are in contact (or nearly so) with the sun's circumference; and which have occasionally given rise to the supposition of lunar mountains in high relief. He thence infers, that all measures of the moon's diameter, when passing over the sun's disc, must be taken with great caution, and with due attention to the proximity of the part measured to the edge of the sun's disc (where alone the distortion seems to take place), otherwise errors and discordances will occur. Those prodigious lunar elevations and depressions, so frequently described in solar eclipses, are seldom or never seen, except at the commencement or termination of the eclipse, or in places near the solar cusps: that is, in those points only which are near the edge of the sun; every other portion of the moon's circumference being comparatively smooth and circular. If this notion be correct, it would seem that the measurement of the solar cusps during an eclipse may be liable also to discordances from this very cause.

Mr. Baily concludes by expressing a hope, that, at the total eclipse of the sun in 1842, and the annular one in 1847 (both of which will be central in Europe), the attention of astronomers will be directed more particularly to this subject, both as to its existence and its cause; and that such a regular system of observations in various places will be adopted, as may best tend to elucidate and explain this very remarkable phenomenon.—*Astron. Soc. Brist.* Dec. 1836.

Wood-joists in English Railways.

The substitution of the continuous wood-joist, bedded in concrete, to support the rail, instead of iron chairs and stone bases at intervals only, is about to take place for the first time on an extensive scale in England. The directors of the Great Western Railway have recently made their first contract for 13,000 loads of timber for this purpose, the whole of which is probably laid down by this time.—Corr.

Masts sent from Canada to France and Egypt.

The French government not being able any longer to satisfy their shipping-demands for masts from the United States, are now in treaty for a supply from Canada. The Pacha of Egypt is also a buyer of the same article in the same market.—Corr.

Present from the British Admiralty to the French Institute.

The Admiralty have recently presented to the library of the Institute of France 269 charts, and 22 works relating to Hydrography and Navigation.

This national present was accepted by the *Académie des Sciences* with marks of special acknowledgment, and was immediately followed by the election of Capt. Beaufort, the Hydrographer of the Admiralty, as a Corresponding Member of the Institute.

New Consideration in future Estimates of the Height of the Atmosphere.

All determinations of the height of the atmosphere founded upon the duration of the twilight, which have been admitted up to the present time, repose upon the hypothesis that the solar rays which define the limits of the phenomenon, have been reflected but once. All have supposed, that after two reflections by aërial strata, the solar light is so much attenuated as to be no longer appreciable. These elements can no longer be admitted into the calculation. Experiments on the polarization of light have, in fact, proved, that reflections, though repeatedly multiplied, still contribute very materially to the dissemination of the sun's light through the atmosphere, and that, in every direction, there are rays, which, after frequent reflection, continue to make a notable part of every pencil of solar light which enters the eye. Further, it will be evident, that by the introduction of this new element into the calculation, the altitudes of the atmosphere will be found to be less than those given by the old method.—ARAGO.

English Weights not yet satisfactorily compared with the French.

EXTRACT from the *Annuaire pour l'An* 1837, *par le Bureau des Longitudes.* Mesures Anglaises comparées aux Mesures Françaises, p. 74.

POIDS.—(Ils ne sont pas parfaitement sûrs.)

Anglais. Troy.	*Français.*
Grain (24e de Pennyweight)	0,065 gramme.
Pennyweight	1.555 gramme.
Once (12e de livre Troy) ..	31 091 grammes.
Livre Troy impériale	0,373096 kilogramme.

Anglais, Avoirdupois.	*Français.*
Dram (16e d'once)	1,771 gramme.
Once (16e de la livre).....	28,338 grammes.
Livre avoirdupois impériale	0 4534 kilogramme.
Quintal (112 livres)	50,78 kilogrammes.
Ton (20 quintaux)	1015,65 kilogrammes.

Français.	*Anglais.*
Gramme	15,438 grains Troy. 0 643 pennyweight. 0,0322 once Troy.
Kilogramme	2,6803 livres Troy. 2,2055 livres avoird.

The high reputation of the members of the French *Bureau des Longitudes* gives to the tables of calculations in the *Annuaire* compiled by them, a value highly esteemed by all lovers of accuracy.

In the *Annuaire* of 1833, the table of English measures, compared with the French, was, for the first time, stated to have been contributed by M. Mathieu, one of the *Astronomes Adjoints* of the *Bureau*, and there was then, for the first time, attached to that part of the table which is given above, the warning note, with which it is headed,—*It is not quite certain that they are exact!* The same table, and the same note, appeared in 1834-35-36, and very recently again in the present year. In no year have they been accompanied by any statement of the degree of inaccuracy, nor has there been, as far as we know, any attempt to correct it. When we ask the *physiciens* of both countries to remove this bastardizing bar from the shield, we do it with a full conviction of the extreme difficulty of the subject, but we request them to call to mind the hourly importance of correct comparison, and that the further neglect of it by England and France, at a time when the United States, Denmark, Prussia, &c., have lately been pursuing and obtaining satisfactory results of a similar nature, is a disgrace of no ordinary magnitude. Till the comparison is actually accomplished, it would be very desirable to know what are the limits of error in the above table; and, not to wait for another year, we should be proud to assist in giving publicity to any statement of them that may proceed from satisfactory authority.

Proportional Value of the present coined Metals in France.

THE value of Gold in proportion to that of Silver is as . . . . 15·5 to 1

Gold	. . to . .	Billon[*],	62·0 to 1
Gold	. . to . .	Copper,	620·0 to 1
Silver	. to . .	Billon,	4·0 to 1
Silver	. to . .	Copper,	40·0 to 1
Billon	. to . .	Copper,	10·0 to 1

Mr. Dunlop elected a Member of the French Institute.

THE Section of Astronomy of the *Académie des Sciences*, have again been called upon for a list of persons who, in their opinion, are deserving the distinction of Corresponding Member: they have presented the following:—

1.	Dunlop . . .	*Paramatta.*
2.	Carlini	*Milan.*
3.	Smyth	*Bedford.*
4.	Littrow. . . .	*Vienna.*
5.	Hansen . . .	*Gotha.*
6.	Santini . . .	*Padua.*

On comparing this list with that presented by the same section on the 14th of November last[†], from which Mr. Baily was elected, it will be seen that Mr. Dunlop maintains his precedence, and that the name of Capt. Smyth has had a most gratifying insertion[‡].

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[*] An alloy of silver and copper.

[†] *Magazine of Popular Science*, Vol. II., p. 483.

[‡] The Academy proceeded to the election on the 30th of January. The number of voters was 49. There were for

Dunlop . . .	25
Carlini . . .	22
Smyth . . .	1
	1 white ball.

Mr. Dunlop was declared duly elected, making the third Englishman elected into this distinguished body within three months.

Red Stains in Marble.

THE examination of the cause of these curious, but, at the same time, injurious appearances, in a commercial sense, has lately occupied the attention of two distinguished *physiciens* of France, MM. Payen and Turpin. The most striking result is, that the stains in the Italian statuary marble, (at least of that of the quarry of Sarravezza,) and in the ordinary white marbles of France, are of a totally different nature, the former being of vegetable origin, and the latter, mineral. We shall extract from some communications lately made to the *Académie des Sciences* by these *savants*, some of the more interesting facts.

ITALIAN MARBLE.

"AT the request of M. Henraux, proprietor of L'Altissima, a mountain in which very fine statuary marble is obtained, I examined, during my visit to Italy, the red stains, said to be ferruginous, which occur not only in the unhewn blocks, but even in others which have been a long time sculptured.

"The observations I made upon the spot, by the aid of reagents, and, directly, by the microscope, appeared to me to demonstrate that all the stains occurred in small cavities full of water, on the surfaces of the rocks and of the blocks in the quarry of Sarravezza, and also in the workshop of a sculptor at Leghorn;—that all these stains were of a very different nature from that which they have hitherto supposed to have been;—that they result, in fact, from an innumerable multitude of spherical globules, red, translucent, organized, each individual of which forms a vegetable rudiment, capable of self-reproduction in suitable media.

"There are several simple means by which these superficial productions can be rendered evident, or be made to disappear, at pleasure.

"I hastened to submit to the investigation of M. Turpin, the specimens which I have brought away, and to request that he would verify these facts, and to determine the genus to which the vegetable product in question may belong. I have obtained from the kindness of this *savant* the promise that he will present them to you himself, with his opinion of these small microscopical beings, and with the coloured figures which he has made from them."—PAYEN.

The following is extracted from the memoir of M. Turpin, referred to by M. Payen, which he subsequently submitted to the Academy:—

"Seen with the naked eye, this substance appears as a tint, or a pulverulent coating, at first of a rose-colour, slightly tinged with violet, which passes into a blood-red.

"On examining it near, it is evident that this coating is owing to a multitude of red points, all very fine, although varying in size; a greater or less quantity of them, on the same spot, determines the greater or less intensity of the always irregular red tint, in the various parts of its extent over the marble.

"This kind of coating, which exists only on the surface of marbles exposed to the humid influence of the atmosphere, adheres but slightly to those on which it is developed, for a moistened hair-pencil merely passed over it is sufficient to detach it and completely clean this part of the marble.

"Its aspect is very different from that produced by the existence of iron in the interior of marble. After the slightest comparison of these two products, there is no danger of confounding them, even when seen at a great distance. The tint, or the red coating of the former, generally covers large areas of surface: its colour is a rose-red, or a blood-red, while the ferruginous stains are very limited in extent, and are of an ochre-yellow, or sometimes an orange-yellow."

After a most laborious examination of the subject, and an elaborate detail of everything relating to it, M. Turpin arrives at the following conclusions:—

"1st. That the pulverulent spots of a blood-colour which sometimes coat the surface of the marble of the quarry of Sarravezza, of which M. Payen has brought away specimens, are formed by the successive developement of the same minute red vegetation described by various authors under the names of *Uredo nivalis*, *Protococcus nivalis*, *Protococcus kermesinus*, *Palmella nivalis*, *Lepraria kermesina*, and *Globulina kermesina*.

"2nd. That this vegetation, in which I have never seen the ordinary *thallus*, is, in some manner, one of the two elementary and constituent organs of the tissular masses of all species of the vegetable kingdom: they are the rudiment (like the *Lipsa*) of the lichens and terrestrial algæ, for it is only necessary to imagine the scattered individuals united together to obtain the equivalent of the ordinary *thallus* of the lichens.

"3rd. That the region the most inhabited by this minute vegetation is the humid surface of calcareous rocks; that it is from this cause that they appear to redden the surface of marble, both unhewn and sculptured, in the same way that other analogous vegetations, the *Alysphæria antiquitatis*, blackens marbles and statues, exposed to the humid influence of the atmosphere.

4th. That inasmuch as it is difficult to detach the *Alysphæria antiquitatis* from statues without injury to the exterior finish which gives them spirit and life, so is it easy to remove entirely the *Globulina kermesina*. Adhering but little to its *habitat*, it may be cleared away in an instant with water, and a brush or sponge. This difference proceeds from the circumstance that the vesicular and seminuliferous globules of the individuals of the *Alysphæria antiquitatis*, are preceded and borne by small coralloïd *thalli*, which fasten themselves to all the inequalities of the marble, while the individuals of the *Globulina kermesina*, limited to one seminuliferous globule, can only adhere by one point of their sphericity.

"From inquiries made by M. Payen, as well as by myself, among various marble-masons and statuaries of Paris, I was disposed to think that the *Globulina kermesina* would not vegetate on the surface of marble in our climate, not one of the parties who was questioned about it, ever having remarked it. I was even led to believe, that this vegetation was not to be met with in the atmosphere of Paris; when a few days since, M. Cagniard-Latour, sent me a red substance, which he had collected from the sides of a garden-pot partly filled with water. This matter, examined in the microscope, was found to be an agglomerate, composed of a prodigious number of individuals absolutely identical with those of the marble of Sarravezza; they were only a little larger, and this may be accounted for by the humid place in which they vegetated having had a great degree of shelter.

"Finally, the calcareous pebbles, collected on the sea-shore, which I have received from Hâvre, and which are perfectly red by the presence of the *Globulina kermesina*, prove that this microscopic vegetation is, everywhere, very extensively disseminated, and that its *seminules* require for a *habitat* and for their developement, merely a surface on which they can rest, being able, like numerous other plants, (*appendiculaires monocotyledons et dicotyledons*,) to obtain the nutritive matter necessary for their growth and their reproduction, in the circumambient and humid medium of the atmosphere."—TURPIN.

FRENCH MARBLE.

"STAINS, similar in appearance to those which annoy the artists and the quarriers of statuary marble in Italy, disfigure the common white marbles of France; they are found in old work, particularly in slabs and squares; and it is frequently necessary to change pieces of large dimensions, in order to restore their original appearance to our monuments.

"One of the most remarkable instances of this spontaneous change, occurs on the great flight of marble steps in the park, at Versailles. Large and numerous stains of a bright red, have there gradually extended themselves over all the steps.

"I am obliged to M. Dastier de la Vigerie, *Ingénieur en chef*, for a most favourable opportunity of examining fragments of many pieces so stained. After exposing them to heat, to reagents, and to microscopical observation, every doubt was removed of the mineral nature of their coloration; it adheres firmly; the greatest part does not penetrate further than abont one or two-twentieths of an inch in depth, between the crystals of the marble; this appears tinted with a fine cinnabar-red, when seen by reflection, and of a yellow-orange, when seen by transmitted light. The peroxide of iron is the only colouring matter of these stains; they contain no trace of the compounds of lead or of mercury, which their beautiful gradations would appear to indicate, nor any organized matter; they, therefore, differ in every respect from the red stains of Italy; it is highly probable, that the only really efficacious means of destroying them, is to remove the surface of the marble as far down as they are observed.

"As to the cause of this phenomenon, and the means of ascertaining its pre-existence, or of destroying it in marble blocks and slabs, I have reason to believe that I am on the right road to their discovery, and I shall have the honour of laying before the academy the results of my researches on this point, as soon as they shall have acquired a character of certainty, or of such probability as shall appear to deserve confidence."—PAYEN.

Railway Legislation—Session 1837.

INITIATORY proceedings have taken place in the present session of the House of Commons for no less than *seventy-seven* Bills relating to Railways. To the surprise of most persons acquainted with Railway transactions, this total exceeds that of the corresponding period of the last Session by no less than *twenty.*

From a comparion of the distribution of the Railway projects over the great divisions of the empire, it would appear that the employment of capital in this direction is increasing in Ireland as well as in England.

At the commencement of the two Sessions the Railway Bills were divided thus :—

	Session. 1836.	Session. 1837.
England and Wales	45	61
Scotland	9	10
Ireland	3	6
Total	57	77

There are not, of course, as many new Railways proposed as there are Bills. Several of the latter are for rival lines, and others are for the extension and amendment of Acts already obtained.

Académie des Sciences, Paris,—Appointments.

M. BECQUEREL has been elected Vice-President of the *Académie des Sciences* for the present year; and, agreeably to the constitution of the Academy, will succeed to the office of President in 1838. M. Magendie, the Vice-President of the past year, is now President for the current one.

The Bonite.

M. ARAGO has received a letter from M. Darondeau, dated on board the *Bonite,* in the anchorage of Puna, Guayaquil, Peru, Aug. 6, 1836. The following is an extract.

"The short duration of the visits of the *Bonite* at the several places at which she stopped, has necessarily prevented any attempt to resolve all the questions which were pointed out by the academy. The observations relating to meteorology and to terrestrial magnetism, will, however, form a very complete series. The diurnal movement of the needle has been observed at Rio-Janeiro, Valparaiso, Lima, and Payta. This last city is situated between the two equators."

Letters-Patent Law Improvement.

MR. MACKINNON[*] has redeemed early his pledge of last session, memorable for the signal failure of all attempts to get even a second reading to this Bill. Leave was given to him and to Mr. Baines[†], on the 14th instant, to bring in a "Bill to alter and amend the Patent-Laws, and for better securing to individuals the benefit of their Inventions." The attack of the monster is therefore renewed; but beyond this fact, which is certainly important, there occurred in the opening of the campaign very little on which any hope of final success could be founded.

Patent-Law Grievance. No. XII.

THE inventors of this country, and the introducers of inventions of other countries into this, were obliged to pay down to the attorney-general and other agents, &c., of the Government during the past year, above £42,000. *What did the attorney-general, &c., in return to them for this vast and oppressive extortion?*

The penalties inflicted on the inventive genius of Britain during the *present* year, up to the 25th ult., in the shape of government stamps and fees on patents, amount to more than £6000 !

N.B. This sum has been paid in *ready money* on taking the first steps, and as many of the inventors are poor men, (*Operatives,*) and a great many others of them persons to whom it would be very inconvenient to pay at least £100 down, they have been obliged to go into debt, or mortgage or dispose of their inventions, either wholly or in part, &c.

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NEW PATENTS. 1837.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second that on or before which the Specification must be delivered and enrolled.——The abbreviation *For. Comm.*, signifies that the invention, &c., is "a communication from a foreigner residing abroad."

January *cont.*

22. Miles Berry, Chancery-lane, *Middx.*, Patent-Agent; for improvements in machinery or apparatus for manufacturing metal screws, part or parts of which are also applicable to shaping metal for other purposes. Jan. 28.—July 28. *For. Comm.*

23. James Hellewell, and Aaron Fearn, Salford, *Lanc.*, Dyers; for certain improvements in the process of dyeing and scouring piece-goods and other fabrics, and in the machinery or apparatus whereby the same is performed. Jan. 28.—July 28.

24. John Springall, Oulton, *Suff.*, Iron-Founder; for improved shoes for horses and other animals. Jan. 31.—July 31.

Total, January...24.

February.

25. James Cook, Birmingham, *Warw.*, Gun Manufacturer; for improvements in gas-burners. Feb. 2.—Aug. 2.

26. William Geeves, Old Cavendish-st., *Middx.*, Gentleman; for certain improvements on steam-engines. Feb. 2.—Aug. 2.

27. Michael Linning, Hill-st., *Edinb.*, Clerk to the Signet, for a certain improved method of operating for the purpose of converting peat-moss, and peat-turf or bog, into fuel, and obtaining from it tar, gas, and other certain substances or matters. Feb. 6.—Aug. 6.

28. John Gemmell, Stockwell-st., Glasgow, *Lanark*, Merchant; for certain improvements in steam-boats, ships, or other vessels, which are partly applicable to other purposes. Feb. 6.—Aug. 6.

29. William Boarder, Bradford, *York.*, Millwright; for improvements in steam-engines. Feb. 16.—Aug. 16.

30. John Walker, Allen-st., Lambeth, *Surry*, Oven-Builder; for an improved method of heating coppers, stills, and boilers. Feb. 16.—April 16.

31. William Stedman Gillett, Guildford-st., *Middx.*, Gentleman; for improvements in harness for draft and saddle horses. Feb. 16.—Aug. 16.

32. Richard Burch, Heywood, *Lanc.*, Mechanist; for improvements in locomotive steam-engines, to be used either upon rail or other roads, which improvements are also applicable to marine and stationary steam-engines. Feb. 16.—Aug. 16.

33. Robert Smith, Manchester, *Lanc.*, Engineer; for improvements in the means of connecting metallic plates for the construction of boilers and other purposes. Feb. 16.—Aug. 16.

34. John Isaac Hawkins, Chase-Cottage, Hampstead-rd., *Middx.*, Civil Engineer; for certain improvements in the application of the products of combustion, in generating and in aiding of steam, for giving motion to steam-engines. Feb. 16.—Aug. 16. *For. Comm.*

35. Henry Elkington, Birmingham, *Warw.*, Gentleman; for improvements in covering or coating certain metals with platina, and also improvements in gilding certain metals, and in apparatus used in such processes. Feb. 17.—Aug. 17.

36. Henry Elkington, Birmingham, *Warw.*, Gentleman; for improvements in steam-engines and in boilers and furnaces, used therein, and for other purposes. Feb. 17.—Aug. 17.

37. John Chanter, Earl-st., Blackfriars, *Lond.*, and of Upper Stamford-st., *Surry*, Esq., and John Gray, Liverpool, *Lanc.*, Engineer; for their improvements in furnaces for locomotive engines, and other purposes. Feb. 17.—Aug. 17.

38. Benjamin Baillie, Henry-st., Cumberland Market, *Middx.*, Metal Frame Maker; for certain improvements in regulating the ventilation of buildings, which he intends to denominate Baillie's Patent Ventilation. Feb. 20.—Aug. 20.

39. John Hardman, Bradford, *York.*, Millwright; for improvements, in steam-engines. Feb. 21.—Aug. 21.

40. Jasper Weston, Dover, *Kent*, Gentleman; for improvements in certain wheeled carriages. Feb. 23.—Aug. 23.

41. John Thomas Betts, Smithfield-bars, *Lond.*, Rectifier; for improvements in the process of preparing spirituous liquors in the making of brandy. Feb. 25.—Aug. 25. *For. Comm.*

42. Thomas Bentley, Cleckheaton, near Leeds, *York.*, Dyer; for improvements in fulling woollen cloths. Feb. 25.—Aug. 25.

METEOROLOGICAL JOURNAL FOR JANUARY, 1837; KEPT AT BLACKHEATH ROAD.

Day of Month		Barom. 9 A.M.	Ther. attch.	Barom. 3 P.M.	Ther. attch.	Thermometer.		Daily Temp.	Solar Var	Rad.	Clouds.		Winds		Direction of Wind		Lunation.	WEATHER, &c.
						Min.	Max.				A.M.	P.M.	A.M.	P.M.	A.M.	P.M.		
SUN.	1	30·596	40°	30·552	41°	27°6	32°5	30°0	4°9	27°	5	10	1	1	N	N		Snow early A.M.; cloudy.
Mon.	2	30·485	40	30·450	43	16·0	33·0	24·5	17·0	15	0	10	1	1	sbE.S	W		Clear A.M.; Afternoon misty and cloudy.
Tues.	3	30·397	43	30·351	43	31·6	38·4	35·0	6·8	31	7	10	0	1	W.	NW		Very thick, close, cloudy day.
Wed.	4	30·436	44	30·425	45	30·7	39·0	34·9	8·3	29	10	10	0	0	SW	S.SW		Fine for an hour about twelve; densely cloudy.
Thurs.	5	30·249	47	30·068	46	33·8	38·5	36·2	4·7	33	10	10	0	1	S	S		Very much overcast; Night clear at intervals.
Friday,	6	29·751	47	29·700	49	32·0	47·8	39·9	15·8	31	7	7	3	3	swbw	WSW	●	*Scud*, with wind; *Cirrus*, *cirro-cum.*, *cum.-stratus*, and *nimbus.*
Satur.	7	29·700	48	29·850	49	36·8	42·9	39·9	6·1	35	0	5	3	2	W	W		Wind very strong; mostly clear.
SUN.	8	30·303	48	30·301	49	29·2	37·5	33·4	8·3	28	1	2	1	1	SW	SSW		Hoar frost; fine *cirri* in groups; cloudy Night.
Mon.	9	30·288	51	30·250	53	31·5	48·2	39·9	16·7	30	10	7	3	3	swbw	WSW		Cloudy, with a gale.
Tues.	10	29·862	53	29·825	54	43·0	49·0	46·0	6·0	43	10	10	3	3	swbw	N		A continued gale of wind till 2 P.M.; Even cold & cloudy
Wed.	11	30·204	49	30·324	48	26·5	32·8	29·7	6·3	26	2	4	1	1	N	N		Sharp black frost: much cloud (*cirro-cum.*) P.M.
Thurs.	12	30·202	46	30·006	46	26·2	36·4	31·3	10·2	24	9	10	1	3	S	SSW		Cloudy; Afternoon snow and sleet, ending in rain.
Friday,	13	29·541	50	29·421	51	32·3	49·0	40·7	16·7	32	10	10	3	3	SW	wbs.NW	☽	Very mild; wind, with *scud*, rainy, Night windy.
Satur.	14	30·025	47	30·150	48	33·4	38·3	35·9	4·9	31	7	5	3	3	Nbw	Nbw		Wind strong from north; flying clouds.
SUN.	15	30·486	46	30·452	47	31·3	37·2	34·3	5·9	30	8	8	1	1	NbE	NbE		Cloudy, *scud.*
Mon.	16	30·425	46	30·380	47	30·0	36·8	33·4	6·8	28	10	10	1	1	W	W		Light snow; cloudy and misty.
Tues.	17	30·364	47	30·328	48	34·4	41·4	37·9	7·0	34	10	10	0	0	—	N		Misty, with drizzling rain.
Wed.	18	30·250	48	30·174	49	37·0	38·0	37·5	1·0	36	10	10	0	0	N	N		Cloudy, and almost dark, with mist and smoke.
Thurs.	19	30·024	47	29·946	47	32·9	37·4	35·1	4·6	33	10	10	1	1	ENE	ENE		Ditto, drizzling rain; black Night.
Friday,	20	29·836	47	29·790	47	32·5	35·0	33·6	2·5	32½	10	10	0	0	E.	E		Ditto, sleet and snow; unusually dark.
Satur.	21	29·748	46	29·683	48	31·2	42·0	36·6	10·8	30	10	10	2	1	S	sbw	○	Clouds broken; air mild; much *scud*; damp air.
SUN.	22	29·480	49	29·376	51	39·8	50·8	45·3	11·0	39	10	10	3	3	S	SW		Wind & rain; *scud* & *cirro-stratus* } high temperature,
Mon.	23	29·515	52	29·475	55	46·5	50·0	48·3	3·5	45	10	10	3	3	SW	SW		Ditto Ditto } very moist.
Tues.	24	29·650	55	29·696	55	45·0	49·5	47·3	4·5	44	7	10	2	1	SW	S.SE		Clouds broken, A.M., very mild; rain at Night.
Wed.	25	29·650	55	29·706	55	42·6	46·0	44·3	3·4	42	10	10	1	1	S.	SW.N		Overcast; *cumuli*, *stratus* & *cirro-stratus*; wet Night.
Thurs.	26	29·682	53	29·680	53	42·0	44·0	43·0	2·0	40	10	10	2	3	E	EbN		Much rain; an inundation.
Friday,	27	29·851	48	29·900	48	36·6	38·3	37·5	1·7	37	10	10	3	2	ENE	NE		Cloudy; much overcast; light drizzling rain.
Satur.	28	29·949	47	29·950	46	33·0	35·1	34·0	2·1	33	10	10	2	2	NE	NE		Sleet and snow; great masses of clouds, with snow, P.M.
SUN.	29	29·878	45	29·815	46	31·7	34·0	32·9	2·3	31½	10	10	1	0	ENE	E.S	☾	Snow & rain; overcast, with *cirro-stratus* and *scud.*
Mon.	30	29·876	46	29·851	48	29·0	41·6	35·3	12·6	29	10	10	2	2	SE	SSE		Light showers.
Tues.	31	30·005	50	30·010	50	40·0	46·7	43·4	6·7	39	7	7	1	1	S	sbw.s		Very mild; *cumuli*, *nimbi*, with a *rainbow* at 3 P.M.
Mean		30·023	47	29·993	48	33·75	40·87	37·32	7·12									

Bar. Max. 30·596 in. on the 1st.
Bar. Min. 29·376 in. 22nd.

Ther. Max. 50·8° on the 22nd.
Ther. Min. 16·0° 2nd.

Lowest point of Rad. 15°, on the 2nd. *(on the snow.)*
Rain fallen, 2·94 in.

ON PROTECTIVE INK AND PAPER.

[The following article is the Report of a Commission appointed by the *Académie des Sciences* from among its members, in consequence of a request from the French government. The Commission consisted of MM. Gay-Lussac and Dulong, of the Section of Physics, and of the whole of the members of the Section of Chemistry; viz. MM. Deyeux, Thénard, D'Arcet, Chevreul, Robiquet, and Dumas. Among these will be recognised some of the most distinguished names in France. The report was drawn up by M. Dumas[*], and, as stated in it, the direction of the inquiry was confided to him and to M. D'Arcet, a selection which reflects the highest credit on the sagacity of those who made it. There would have been little difficulty in abstracting this document, and thus, for the attainment of a little brevity, to have presented a shrivelled and diminutive copy of the original to our readers. Our respect for the members of the Commission would alone have made such a task extremely repulsive; and if we had so decided, such among our friends as really delight in the spread of scientific truth, and in the dispersion of the obstacles which impede its diffusion, must have lost, at least through the means of our agency, the gratifying spectacle of science encountering and eventually triumphing over official prejudice, if not, as we suspect, over official corruption. State-blindness to the light of science, and frauds in stamps and stationery, are not so utterly unknown among us, as that we can regard such conflicts and victories with indifference.

The falsification of deeds is, however, we believe, but rarely attempted in Britain, while we have grounds for believing, independent of the testimony, direct and indirect, in the Report, that in France it prevails to a very frightful extent. We can vouch for the truth of the following instance.

A year or two ago, an English gentleman carried with him to Paris a power of attorney from a friend in England. The object of it was to effect the conveyance of some property which the latter possessed in France. This property had been regularly entered in the registers of the Government, but, on comparing the name in the power of attorney with that in the register, there was a considerable discrepancy. This happened in an early state of the transaction, so that, though notices of the transfer had been given to the proper parties, there was still a considerable interval to elapse before the time appointed for the execution of the transfer. The French attorney employed undertook to send back the power to England, with instructions for the correction of the discrepancy. It was so sent, a correction was made, and the usual notarial forms to perfect the validity of it were all gone through. The corrected instrument arrived in Paris in excellent time; it was there twenty-four hours before it was necessary to present it. It happened that during the evening of the day on which the power returned, the English gentleman was laughing with a friend, on the national incapacity, as he termed it, of the French to spell foreign names correctly; he related the blunder in the register, and produced the documents to verify his story. To his astonishment, his friend remarked that the new power was more discordant than the previous one. The French attorney was sent for in the greatest haste; and was most reluctantly obliged, not only to admit the fact, but to acknowledge that to him alone must the blame be imputed, and that there was now no time for a second dispatch to England. The case was desperate; the opportunity of transfer lost, was in this case gone for ever. As may be imagined, every means of getting over the difficulty was discussed, and, as the Englishman thought, exhausted, without a remedy being found. He saw it was not the dot of an *i*, or the running up of it into a *l*, that would relieve them. Such *homœopathic* remedies were perfectly hopeless in their case. At a late hour the parties separated, the French attorney taking the documents with him.

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[*] A sketch of this gentleman, as Lecturer on Chemistry at the Sorbonne, is given in this Magazine, Vol. I., p. 177.

The Englishman, on the following morning, had waited long after the man of law had appointed to meet him; the hour was approaching when he knew that, by official regulation, the production of the power of attorney at a certain *Bureau* must take place, and be compared, to complete the affair. No attorney appeared, nor any messenger from him, and the most perplexing anticipations began to fill the imagination of the Englishman; he felt the utmost embarrassment as to what he ought to do. He was happily relieved, in nearly the last minute, by the entrance of the attorney, calm, smiling, pouring out the ordinary apologies for being a little late; and, without referring in the most distant manner to the anxious deliberations of the preceding evening, he merely entreated his client to hasten with him to the *Bureau*. "*But the power of attorney?*" "*Don't trouble yourself about that; I have it, you know. Come, we haven't a moment to spare.*" They descended to the street. The short distance they had to go was not favourable to further inquiry, and no explanation was volunteered. In the *Bureau*, the register and the power of attorney were produced,—compared,—not the slightest discrepancy! the client looked astonished towards the attorney, but the eye of the other *happened* to be ranging in a very different direction; it certainly betrayed no consciousness of anything unusual. At the *porte-cochère*, where the parties separated, the only explanation of the attorney's serenity was volunteered in the following assertion:—"*I found the instrument this morning not quite so bad as we thought it last night.*" The Englishman felt it was much worse, for, during the night, the whole signature had been effaced, and restored so as to answer the purpose. The French attorney in question has a large practice, and a fair average character in Paris.

The Report itself will present to many persons an interesting, and probably unexpected, view of the numerous difficulties which must be surmounted, before a protective ink or paper can be obtained which will defy the ingenuity, and defeat the perseverance, of the fraudulent. There will be found distributed in every part of it suggestions which may be serviceable for other purposes than those which were the objects of the Commission, and a spirit of practical appliction and indefatigable inquiry pervades the whole.]

SEVERAL years ago, a Committee chosen from the Academy examined in the most careful manner certain means of preventing the falsification of public and private deeds; at the same time, and with equal attention, they deliberated upon the methods by which the Government might put an end to the fraudulent bleaching of stamps which had been used, an operation known to be carried on to a large extent, and by which the same stamps could be used several times over, to the great injury of the revenue.

To prevent the falsification of deeds, both public and private, the Commission proposed the use of an *indelible* ink, made by a solution of Indian ink in water acidulated by muriatic acid.

To prevent the bleaching of used stamps, they advised that the paper of all stamps in future should be covered with an engraved, engine-turned, pattern, printed with a *delible* ink, the base of which might be common ink itself properly thickened.

Subsequent experience has confirmed the propriety of these recommendations. Characters written upon common paper with the indelible ink have not only resisted every attempt at falsification, made by persons interested in establishing other protective methods, but, in addition, the paper on which they have been written has suffered no appreciable change from them during six years. The delible designs printed on common paper have been perfectly effaced by the means which destroy ordinary writing, when the designs were printed with the sediment of thickened ink, as the Academy recommended.

The task of the Academy, therefore, appeared to be completed, since the only difficulties which remained to be surmounted were purely technical or official, and these were not within its province. But, six years have passed away since the Academy sanctioned the methods which have been described, and there is not yet any ground whatever for asserting, that the Report of their Commission has produced the slightest useful effect.

May it not, therefore, be supposed, that when a guarantee against fraud is requested, the real damage done by it to society is exaggerated? May it not be supposed, that when a preventive is offered and deliberated upon, there is a disposition, before incurring the necessary inconvenience and expense, to yield too much to rigid calculation? and that, influenced by its results, the means of security which, in the first instance, were so earnestly solicited, are finally estimated at a price too low?

There is no doubt, that falsifications of deeds, &c., are, in general, detected when they appear on commercial papers, or in documents in which individuals are interested; but it ought not to be forgotten, that they frequently escape the eye of justice in official instruments, in passports, or, generally, in papers to which a short time only can be dedicated to their verification.

But if every alarm should subside on the recollection that the terror of detection by chemical investigation, and the threat of an infamous punishment, are always present to deter the fraudulently disposed; still these alarms will be found to return with increased force, when we reflect on the extreme facility with which common ink may be discharged from common paper, and in the ease with which chemical agents necessary for these frauds can now be procured. Temptations are thus perpetually present to minds already perverted, and to which, sooner or later, they may succumb. But if we admit that every fraud is discovered, that every knave is exposed to the public eye and punished, that public and private property is guaranteed, and that the administration of the laws is successfully applied; still is the moral sense satisfied? Certainly not: it would not the less search out means, which, rendering frauds impossible, would present insurmountable obstacles to falsification, or which, rendering it extremely difficult to accomplish, would oblige the falsifier to undertake tedious experiments and long preparations, during which a salutary reflection might occur, and lead him back to better pursuits.

It is probable, that considerations of this nature influenced the Minister who requested from science a protection from so deplorable an abuse of the light which science itself has diffused throughout society, and induced him in 1826 to request, as the primary requisite, the discovery of a means which should render the falsification of public and private deeds altogether impossible. But, at the same time, he called the attention of the Academy to a fact of another kind, viz., the bleaching of stamps which had been already used.

Some years have elapsed since the Academy complied with the request of the Minister of Justice, and communicated to him the results of their experiments on these two points. Since that time, the Government seem to have lost sight of these important subjects; a letter, however, recently received by the Academy from the Minister of Finance, exhibits a wish to carry into execution the advice of the Academy with regard to the manufacture of stamps.

But this Minister, no doubt convinced that the indelible ink published by the Academy is sufficient to stop future obliterations, appears rather impressed with the necessity of protecting the interests of the Treasury, than with any desire of preventing the falsification of deeds, either public or private. He therefore consults the Academy on the labours of the *Direction de l'Enregistrement et des Domaines*, and he inquires if the paper which this Board has produced will render impossible the bleaching of the stamps after they have been once used, and secondarily, the fraud by alteration of the written character; thus placing in a subordinate rank the question which the Minister of Justice considered to be the most important.

The Academy readily perceives, that in giving to the Minister of Justice the means of preventing falsification, and secondarily, that of arresting the bleaching the used stamps, it will also resolve the questions which the Minister of Finance has put to it. In fact, the operation of bleaching stamps cannot be carried on but in the large way, and by means which must be essentially economical; for a sheet of stamped paper costs something, and when bleached must be sold for less than the new stamp: a slight obstruction would therefore annihilate this trade.

With the falsification of public and private deeds the case is different. To these the greatest possible obstacles should be opposed; for by them the honour and the property of individuals, and the public tranquillity itself, might be constantly menaced. All the resources of science and art should be called in aid; for it is not mere economical bleaching which is to be contended with, but it is successfully to counteract the ingenuity of parties whose interest is greatly excited, and who do not hesitate to devote much time, money, and skill, to the accomplishment of their fraudulent intentions.

It is for this reason that the present Commission, like the former one, have thought proper to consider first the question of frauds in writing, and then to proceed to those of the paper. They felt that it was impossible to confine themselves within the limits pointed out in the letter of the Minister of Finance, a general report appeared to them indispensable, and they devoted to it a long and circumstantial labour, the direction of which has been more particularly entrusted to M. D'Arcet and myself. The Commission have therefore concentrated every light that could be thrown upon the subject. They have not only collected the evidence of the officers of the Government, but also that of every individual whose experience qualified him to be of service.

It was not until the moment when, enlightened by extensive and repeated discussion, and when every member of the Commission, composed of MM. Gay-Lussac, Dulong, and the whole of the Section of Chemistry, became of one common opinion, that the Commission felt they could submit to the Academy a general Report on the questions which were proposed to them, or which arose during the progress of the deliberations and the experiments.

SECTION I. *Examination of the Paper proposed by l'Administration de l'Enregistrement et des Domaines.*

THE paper of the stamps actually in use has three distinctive marks, viz., in the middle of the sheet are the arms of France, water-marked, on the upper left-hand corner a colourless stamp, and on the upper opposite one a coloured one in unctuous ink.

Nothing can be more illusory than such a system of precaution.

In fact, all *delible* characters, that is to say, all written with common ink upon this stamped paper, can be totally effaced with great ease, without leaving any trace of their existence, and without any of the three distinctive marks on the sheet being in the smallest degree altered.

But, if the marks were delible, *i. e.*, if they were executed with the same ink as that of the characters, or better, if the stamped paper were were entirely covered with a design printed in common ink, the written characters could not be removed without effacing the design itself, and from the moment that took place, the stamped paper would lose its distinctive characteristic,—it would no longer exist.

So that, instead of furnishing the stamped paper with *unalterable* marks, it, on the contrary, should be prepared with those which would be *alterable under certain conditions.* So that the paper actually in use possesses nothing whatever which can prevent the fraudulent bleaching, and, for a stronger reason, nothing whatever which can prevent fraudulent writing. The Board knows this; but they wish to combine the precautionary system proposed by the former Commission, and the employment of their own paper made by hand; and this has given birth to the series of attempts which have been laid before the present Commission by the Minister of Finance.

The Commission had, in fact, proposed to thicken the sediment of common ink, and to print with it, on the paper intended to be stamped, a design engraved on a cylinder of copper, by means of the rose-engine. Paper so prepared, and furnished with a colourless official stamp, would have given to the Government the most perfect guarantee. The bleaching would have been instantly put an end to.

But the Board of Stamps, who have constantly employed paper made sheet by sheet, called *hand-made* paper, hesitated, when they saw that the mode of printing pointed out by the Academy required the use of *machine-made* paper, in endless sheets; and were so strongly prejudiced against an innovation which appeared to them so extensive, that they have been inquiring if they could not make use of the process proposed by the Academy, without giving up their hand-made paper.

They have, in consequence, sought after a means of printing the paper in sheets; and, after rejecting the copperplate and lithographic processes, as too costly, they have contented themselves in using the ordinary means afforded by common printing, or typography.

So that, while the Commission, advising the employment of a design engraven on a copper-cylinder, found itself led to the recommendation of the use of the endless sheet, the Board, in seeking to preserve its customary paper, made in single sheets, found itself conducted, in its turn, to the use of a design cut in relief, and worked by the printing-press.

The point which decided the choice of the Commission was, the certainty, derived from processes in the manufacturing arts, that an aqueous ink can be printed with, economically, on endless sheets, by means of cylinders. The Board conceived that they would be able to combine the use of the typographic process with the economy of press-work; but in giving up the endless sheet, they have been obliged to modify the ink proposed by the Academy. In fact, when it was attempted to pull off proofs from characters in relief with aqueous ink, the results were so imperfect, that the necessity of modifying the ink was immediately perceived. As is well known, the typographical printer uses an unctuous ink, and, very naturally, the Board sought, by the introduction of an unctuous body, or of a varnish, to remove the difficulties which embarrassed them.

But as common ink ground up with varnish or an unctuous body would resist too much the action of chlorine, and, in general, of all those agents which affect it when pure, it became necessary to correct this inconvenience: this was accomplished by means of a large addition of chalk. Therefore, the ink of the Board became ultimately a composition of chalk, of ink-sediment, and of varnish: this we shall call *delible varnish-ink*.

Chlorine and the acids act upon this composition the same as upon common ink; they efface it simultaneously with the characters which are superposed; and in this respect, the numerous trials which the Board have caused to be made have most satisfactorily solved this question, and produced an ink most successfully possessing a delibility *equal to that of* common ink.

But on the very first examination of the specimens which the *Minister of Finance* submitted to the Commission, and which were prepared with a design printed typographically with the delible varnish-ink, three very serious objections presented themselves to the use of this ink, and to the kind of printing which rendered it necessary.

As to the printing process used, it is well known that when an impression is made by means of characters in relief, the paper is indented in such a manner, that if the characters were not inked at all, their configuration would not be the less marked upon the paper, only it becomes evident by hollows; in all such parts the paper is rendered denser and smoother. It is evident that this indentation would resist all the agents which efface common ink or the delible varnish-ink, and that it is quite sufficient to guide the hand of the fraudulent imitator who might attempt to reinstate the effaced design.

In fact, the Academy may convince themselves, by examining the specimens which we place before them, and in which, after having removed in the most careful manner every vestige of the design, by means of chlorine, of acids, and of alcohol, the indentation remained so perfect that the smallest lineaments of the design continued to be visible. A practised hand could reinstate their primitive tint, by following each trait with a pen, the task being remarkably facilitated by the nature of the design; a circumstance to which we shall return further on.

It is true that such work would require time, and consequently be too costly to be at all applicable to the fraudulent trade in bleached stamps; but the presence of the varnish which enters into the composition of the ink used by the Board, tends to remove this difficulty.

We shall remark, that, in order to observe the effect of the indentation, we treated the paper with chlorine and acids to remove the ink and chalk, and then with alcohol to remove the varnish. But this treatment with alcohol would be useless to the falsifier, and he would take care not to have recourse to it. For, after having destroyed the ink of the design, he would find again every trace, either by the indentation, or by the yellowish tint left by the varnish, or by the transparency communicated by the latter to the paper.

These circumstances will explain to the Academy how very easy it has been for us, after having written upon the paper submitted by the Minister of Finance, to efface the writing entirely, and then to reproduce the design either wholly or partially. Having given similar sheets covered with characters written in the usual manner to persons in practice, and good hands, they effaced any words that we pointed out,—they restored and refitted such portions of the design as the bleaching had destroyed, and—they returned the sheets to us in such a state, that any word which had been effaced could be replaced by any other word. Of these facts the Academy may judge for themselves, by examining the specimens which we place before them.

It is thus as much by the effect of the indentation as by the presence of a yellowish varnish in their delible ink, that the Board of Stamps have produced, it may be said, nothing to prevent partial falsifications. These are made to require, without doubt, a little more time, and a little more skill; but, nevertheless, the Commission is led to believe that *the adoption of such a paper would diminish but little the number of frauds.*

What immense injury would also result from the sale by the Government, in its official character, of a prepared stamped paper which should give an illusory guarantee only! How many falsifiers would be seduced by the notion that their crimes would be the better concealed,—would have a superior chance of passing unpunished, when on the simple aspect of the protective paper on which they had been exercising their culpable skill, the interested parties in the first instance, and subsequently, in certain cases, both judge and jury, might be disposed to reject every suspicion of fraud! But, if the paper submitted to our examination does not offer the guarantees which every honest man would wish to see combined in instruments which must become the securities of honour and property in private life, there remains for investigation the inquiry, Will it satisfy the principal want of the Board of Stamps, and safely protect the interests of the revenue?

A sheet of stamped paper which has been used, and afterwards purchased to be bleached and re-sold, can have been subjected to an operation of but two or three farthings in cost, in order that the trade may yield a profit. It therefore follows, that the smallest delible design would be sufficient to destroy the traffic, if the design must be reproduced by hand after the bleaching.

But would it be necessary to reproduce the design by hand on the paper proposed by the Board? No, certainly not.

If the progress of chemistry multiplies the difficulties at every step, in the search of securities against the falsification of writings, the progress of the imitative arts, on their side, raise up a crowd of obstacles, destructive of the greater part of the processes used.

Thus, in the simple fact of the Board having introduced a varnish into their ink, the Commission see the possibility of transferring the design to a lithographic stone, and, consequently, of reproducing indefinitely the identical design on the paper of old stamps which may have been bleached; a result, which also can be obtained by a less direct means, and which we shall notice hereafter. The bleaching of the used stamp-paper would then indeed cost a farthing or two more than at present, and this would be the total amount which the Board would gain by changing its system of manufacture.

The Board need not trouble itself to abandon its present course. Besides, of what use would it be, if, as they tell us, a law demanded by the public interest will in future place such misdemeanors in the rank of crime; everybody will reply as we do, that if a law would prevent them, it should be confined to the declaration that the bleaching of the stamps in circulation is become criminal.

After having rejected their mode of printing, on account of the indentation that it produces, and their kind of ink, from the varnish it contains, there remains to be examined the species of design selected by the Board of Stamps, not as a work of art, but with regard to the difficulties which it throws in the way of falsifiers, and of the bleachers of used stamps.

This design has been executed by a very complicated process, but which is founded on the ingenious arrangement by the aid of which M. Colas has engraved the plates in the *Trésor de Numismatique*. It is known that this kind of engraving is performed by a machine, which draws parallel lines on the copper-plate, receding from each other in the parts where light is desirable, and approaching to form the shadows, but which run without interruption from one side of the plate to the other.

The Board has thought proper to place in the middle of the sheet of paper a figure of Justice seated, about 3½ inches high, and about the same at the base. This figure may be surrounded by any design whatever.

The type of this figure may be found in the centre of the usual colourless stamp; it was originally taken from a bronze-plate, on which M. Galle had cut in relief the model of a seated figure of Justice; by its means, the process of M. Colas produced an engraving on a plate of copper.

Proofs of this latter plate were taken, and transferred to wood, and, by the common means of wood-cutting, a similar figure was obtained on the block, but in relief. From this block, casts were taken, which afterwards served as moulds to produce plates in relief similar to the block, and which were necessary when the printing press was to be used.

The wood-cut being once obtained, as many typographical plates could be obtained as were desired, and in a degree identical with the model. But does the cutting in wood faithfully re-produce the copper furnished by the machine of M. Colas? It is quite impossible that it should. At least, in the attempts produced to us, we cannot recognise that delicacy and purity which characterize all the productions of M. Colas; and further, the several casts are certainly not identical with each other. So that, although we might expect, with regard to art, very successful results from the exertions of our most skilful engravers in relief, the kind of engraving adopted by the Board produces a grave absurdity,—the clear and regular work of a machine is by means of a transfer laid upon a block; this block is then cut, cast from, and the matrices thus produced furnish the plates from which the design is printed. A series of transformations thus takes place, which converts the original design into a spiritless copy. At each succeeding step it suffers a further deterioration.

Thus, the single fact of desiring to preserve the hand-made paper rendered typographical printing necessary, and this again required such peculiar conditions, that the process with which M. Colas has recently enriched the arts has entirely lost its merit.

But let us lay aside the deterioration which the figure in question must suffer in the transfers to which it is submitted. Let us suppose that the figure of Justice is printed directly on the stamp, with the copper-plate executed by M. Colas, and that this figure, although, in this case, more difficult to be re-produced, in consequence of its delicacy and clearness, might be advantageously replaced by a design of another kind. In a word, when the Commission proposed the employment of a cylinder engraved by the rose-engine, it was because they conceived that designs the most difficult of imitation were not always those in which personages are introduced, for the absence of all symmetry in them renders comparison difficult and equivocal; they preferred a simple species of design, produced by lines crossing each other at determined angles, and thus producing a multitude of very small figures, perfectly identical, and (because the eye can include a considerable number of them at a glance,) easily compared with each other.

The Commission feel confirmed in this opinion, and submit that, after it is seen with what facility the figure of Justice has been re-produced upon the paper submitted to their examination, though this figure had previously been entirely effaced, it is impossible for to entertain the slightest doubt.

The Board, in preferring to all other considerations the employment of their hand-made paper, have, therefore, placed themselves in a position which successively robs them of every advantage presented by the various processes which they proposed to adopt.

The typographical process indents the papers; if this be cured by pressing, the new delible ink will not the less assist in re-producing the design after the bleaching, in consequence of the varnish which it contains; and finally, typography, which prints imperfectly the designs of infinite delicacy which the Commission had in view, has compelled the Board to prefer a coarser design, which can be re-produced by hand with a deplorable facility. Their process will not, therefore, prevent falsification, and, as the ink they employ, and the design they have chosen, are favourable to lithographic transfer, it will not interrupt the bleaching of the used stamps.

Your Commission, having thus examined and reasoned upon the processes which the Board have employed to obtain the two-fold object they had in view, proceed to lay before the Academy the results to which they have thought it their duty to limit themselves.

SECTION II.—*Bleaching of used Stamps.*

As has been already observed, nothing would be more easy than to put a stop to the bleaching of used stamps, in the way it is at present practised; the simplest delible design would be enough for the purpose.

But the difficulty does not lie here; for, if the design adopted can be transferred to the lithographic stone, it may be reinstated with great facility upon the bleached paper, and thus the bleaching trade would re-commence.

It may be said, that lithography prints its proofs with an unctuous ink, which would be indelible, and that the fraud would be instantly detected. To this it may be unanswerably replied, that the delible varnish-ink which the Board has prepared, is unctuous and delible, and there is no doubt of its being applicable to lithographic printing.

Therefore, as typography and lithography can make use of the same kind of ink, it may be asserted, that every impression made by typography may become, in the hands of the lithographer, a type susceptible of being multiplied to infinity, and at a very low cost.

We have only hitherto remarked on the facility which the unctuous nature of the ink would furnish to the party who should propose to obtain a transfer of the design of the new stamps proposed by the Board, on the lithographic stone; but we ought to go further, and insist that *this design*, even if it were printed with an aqueous ink, would not be the less favourable to a faithful transfer. What would be easier, in fact, than to charge each line with a pen filled with an unctuous ink prepared for transferring?

As demonstration of this, it was sufficient for the Commission to entrust one of the proofs of the design to an able lithographer, in order that he might make a trial of charging some parts, and rendering them fit for a transfer. We submit the result of this experiment to the Academy, and they will think with us, and with the artist who has thus assisted, that, with a little time and expense, it would be easy to re-produce the whole figure.

It is true, it has been objected that, after having bleached the used paper, it will not only be necessary to print the same design, but it must also be printed with delible ink, and be fitted exactly to the colourless stamp. To this it may be answered, that the delible varnish-ink used by the typographer is equally accessible to the lithographer, and the accordance of the colourless stamp with the newly-printed design will be, if not quite exact, at least sufficiently so, particularly when a colourless stamp is considered whose outlines may have been blunted by the bleaching.

Therefore, in order really to prevent the bleaching of used stamps, not only as it is practised at the present moment, but, if we rest content with merely making it a little more difficult, as it may be practised in future, the three following conditions must be obtained and combined.

1. The paper should be covered with a design printed in an aqueous ink, which cannot be directly used in transfer.

2. The design should be of such delicacy as to make it impossible for the most skilful hand to charge the lines with an unctuous ink.

3. The design should be changed every year, in order to prevent all temptation, by means similar to those used in the production of it, to imitation.

Up to the present time, typographical processes have produced nothing which can justify the belief that they can be used in satisfying these conditions. The most simple means would therefore be, to return to the endless sheet, the engraved cylinder, and the thickened ink, already proposed by the Academy. But in perceiving themselves irresistibly returning to this point, the Commission have felt that they ought not to confine themselves to mere advice, but to demonstrate, by incontestable facts, the perfect efficiency of their proposals.

They are confident that common writing ink, thickened with moulders' plaster, furnishes an article perfectly proper for printing with cylinders.

They have the honour of laying before the Academy the results of the experiments they have made with this ink by means of a cylinder.

The Commission proceeded further, and being disposed to remove, as much as in their power, the difficulties which have distracted the Baard of Stamps, they considered whether it would not be possible to preserve the advantages of an aqueous ink, and not have recourse to the employ of machine-made paper.

They conceived that the use of the copper-plate printing-press deserved to be *examined for this* purpose. In fact, the Commission have taken off, by a press of this kind, in aqueous ink, proofs of the most delicate designs they could procure, and succeeded perfectly well.

The Board might, therefore, if the cost of printing were not an objection, and which we cannot discuss here, adopt the use of a similar press, and by so doing might substitute an engraved plate for the cylinder,—replace the machine-made paper by sheets pasted end to end, and make use of common ink, maintained at a proper thickness, without any unctuous or resinous addition, and avoiding varnish.

The Commission finally inquired if it would not be possible to apply the typographic process more successfully than it had hitherto been done, in order to accomplish the object intended.

They found but one means by which typography could produce a design which could prevent the bleaching of the used stamps. This consisted in printing the design with two inks, the one delible, the other indelible.

Suppose, for example, that the ground of the design was of a lace-pattern, and that a number of small circles were distributed here and there over the whole surface; suppose that each small circle was formed of a semi-circumference delible, and a semi-circumference indelible; with such precautions, the bleaching of the used stamp would certainly be impossible.

In fact, the bleaching having removed the delible portions of each circle, let us inquire how they may be restored. By hand, it would be too costly; and by printing, it would be impossible, for the coincidence of the semi-circumference preserved on the paper and that which must be added, could not be accomplished by any means.

In this system, it would be of little consequence whether the delible ink were unctuous or aqueous, for the process of transfer could not be used.

To carry on a fraudulent trade in such paper, an establishment would be necessary in which all the processes of the Board must be imitated; this could not be done without that co-operation of willing agents, and species of publicity, which render such frauds impossible. Besides, who would risk the heavy expense which would be necessary for such an undertaking, when the sale of the article would be so surrounded with difficulties, and be constantly threatened with severe punishment by the law?

[To be continued.]

A POPULAR COURSE OF CHEMISTRY.

X.

NITROGEN.

THIS singular elementary form of matter may be very readily procured by the following simple process:—

Fill a soup-plate with water, and float on this a little saucer or cup of earthenware, holding a bit of phosphorus about the size of a large pea; have ready at hand a deflagrating jar, (that is, a glass jar of the shape of a bottle when the bottom is removed,) holding about three pints: now, by means of a heated wire, kindle the phosphorus, and immediately invert over its flame the deflagrating jar without the stopper; let the jar stand in the plate, and then put in the stopper; you will find the combustion of the phosphorus go on for some time; a vast quantity of white fumes are evolved, the water rises inside the jar, and at length the combustion of the phosphorus ceases.

Let the apparatus remain at rest for a few minutes, and you will find the white fumes vanish; they consist of *phosphoric acid*, which is rapidly soluble in water: there now remains in the jar a gaseous body, which is *nitrogen*, very nearly pure. Get the pneumatic trough ready, and now sink the plate beneath the water, and remove it. The next thing to be done is, to get the little saucer out of the jar; and this you may easily do, by shaking the jar slightly from side to side, so as to splash some water into the saucer, and so sink it to the bottom of the trough: this being done, slide the jar on the shelf of the trough, and take another jar nearly the same size, fill it with water in the trough as usual, and transfer the gas in the first jar into it; and then transfer it back again, and so on three or four times. The object of doing this is to wash the nitrogen from any adhering *phosphoric acid;* and you will now find the nitrogen perfectly clear, transparent, invisible, and very nearly pure. Transfer it into as many small stoppered bottles as it will fill, and set them aside for the following experiments.

Nitrogen gas is chiefly characterized by negative properties: it has no colour, no smell, no taste, no action on colours, is not absorbed by water, is not inflammable, and will not support life.

Open a bottle of nitrogen, apply it to the nostrils, and you will find it inodorous, provided that it has been well washed; and remember that for these experiments such must be the case. Now inhale a little of it from the bottle, which may be safely done, and you will find that it has no taste.

Take another bottle of the gas, open it, and place in it a slip of litmus paper, the blue colour of which undergoes no change; now put in a slip of turmeric paper; its yellow colour undergoes no change; so that you may conclude from these two experiments, that nitrogen is neither acid nor alkaline, nor capable of destroying colour.

Pour about an ounce of water into the bottle, insert the stopper, and shake the bottle for some minutes, and you will find that after so doing you can remove the stopper as readily as at first. This shows that nitrogen is not absorbed by the water; for if it were, the stopper would become firmly fixed in its place by the pressure of the external air, as explained when speaking of chlorine, Vol. II., p. 292.

Any small animal soon dies if put into a bottle of nitrogen, but there is no occasion for you to make this experiment; the fact is established beyond a doubt, and *true* chemists are always unwilling to make either wanton or cruel experiments.

Now, from the circumstance of nitrogen being incapable of supporting life, the term *azote* has been applied to it, and indeed it is in common use in some schools of chemistry. The term azote is of Greek derivation, and signifies *privative of life:* this term is highly objectionable, because all other gaseous bodies are privative of life; so that this cannot be considered as a marked and peculiar property of nitrogen; you will find hereafter, that nitrogen, when *mixed* in a certain proportion with oxygen, is absolutely essential to life, and no other gas can be substituted for it.

Azote, therefore, is a bad term, and I prefer that of nitrogen, which simply implies that it is a generator of nitric acid, as our future experiments will fully demonstrate.

Nitrogen is a perfect non-supporter of combustion; *nothing*, not even the most inflammable bodies, will burn in it. Immerse a lighted taper into a bottle of the gas, it is extinguished as effectually as though you had plunged it into water. Take a small deflagrating spoon, containing a little globule of potassium; heat the spoon in the spirit-lamp, until the metal burns with a vivid red flame; then quickly plunge it into a bottle of nitrogen; the burning metal is extinguished. Take another spoon, containing a bit of phosphorus; heat it in the spirit-lamp, until it burns vehemently, then plunge it into another bottle of nitrogen, and the burning phosphorus is extinguished.

Now, these few experiments will be sufficient to give you a notion of the negative characters of nitrogen; you may consider it almost as an inert body; it is rather lighter than common air; 100 cubical inches weigh 30·16 grs., and its specific gravity in reference to hydrogen is as 14 to 1.

I must now inform you regarding the theory of its evolution in the process that we set out with.

Phosphorus was burned in a confined portion of air in the deflagrating jar. Now, atmospheric air is a *mixture* of oxygen and nitrogen gases; the former united with the phosphorus to form *phosphoric acid*, which was absorbed by the water; and the rise of the water in the jar indicated the bulk of oxygen thus abstracted, leaving the nitrogen; so that this experiment may be considered as a kind of rough analysis or decomposition of atmospheric air.

This is not the only method of procuring nitrogen; but it is, perhaps, the readiest. You may procure it by taking a tall glass jar, moistening its interior with water, and then shaking some clean iron filings into it, so that they may stick to the sides of the glass; invert this into a plate of water, and leave it for a day or two, when the bright iron will begin to *rust*, or, chemically speaking, it will combine with the oxygen of the air in the jar, forming *oxide of iron*; the water in the plate will slowly rise in the jar, indicating the bulk of oxygen thus absorbed or combined with the iron; and you will find nitrogen left behind if you transfer it to the pneumatic trough, and examine its properties.

A great degree of doubt exists amongst chemists as to the elementary nature of nitrogen; many experiments have been made upon it, to test its compound nature, but hitherto no proofs have been offered of its decomposition, and we are therefore compelled to acknowledge it as an elementary or simple form of matter, which as yet resists the utmost exertions of analytical skill to decompose.

We know nothing whatever regarding its ultimate nature; I therefore must discuss its history, unbiassed by any hypothesis or theory.

I will now direct your attention to the compounds which nitrogen forms with oxygen. These are very important, and *five* in number: they are called, respectively, *nitrous oxide*, *nitric oxide*, *hyponitrous acid*, *nitrous acid*, and *nitric acid*. These are all perfectly well-defined *chemical compounds;* but there is an important *mechanical* mixture of oxygen and *nitrogen*, constituting *atmospheric air*.

In the compounds of oxygen and nitrogen, you will have an excellent opportunity of seeing how regularly and how definitely the two elements combine; that, although they combine in five proportions, yet the proportion of nitrogen remains unaltered throughout them all; but the oxygen adds on in proportions which are simple multiples of the first or equivalent proportion. This matter will perhaps be more intelligible to you from the following statement:—

The equivalent number of *nitrogen* is = 14; that of *oxygen*, = 8. Now, in the first compound that they form, 14 of nitrogen and 8 of oxygen combine to produce 22 of *nitrous oxide;* the second, 14 nitrogen + 16 oxygen = 30 *nitric oxide;* the third, 14 nitrogen + 24 oxygen = 38 *hyponitrous acid;* the fourth, 14 nitrogen + 32 oxygen = 46 *nitrous acid;* and in the fifth, 14 nitrogen + 40 oxygen = 54 *nitric acid;* the proportion of nitrogen remains the same throughout them all,—viz. 14, or 1 proportional, whilst that of the oxygen is as 8, 16, 24, 32, 40, or as 1, 2, 3, 4, 5 proportionals.

In atmospheric air, a mere *mixture*, remember, the case is different; in it, *two* proportionals of nitrogen $14 \times 2 = 28$, are mixed with one proportional of oxygen = 8.

Having premised thus much concerning these matters, let us now proceed to the examination of the first compound,—namely,

NITROUS OXIDE.

YOU require for the production of this gaseous body, a few ounces of a salt called *nitrate of ammonia:* this can be purchased, but you can easily make it; and if you wish to gain proficiency in chemistry, you should never lose an opportunity of making an experiment.

Dilute three or four fluid ounces of pure nitric acid with about as much water; place the diluted acid in a large clean basin, and now add to it, lump after lump, some *carbonate of ammonia*, or *volatile salt*, which is a very cheap and well-known substance; as it touches the acid, a violent effervescence ensues, due to the expulsion of the *carbonic* acid of the carbonate by the nitric acid, which has a strong affinity for the ammonia. Take care that this effervescence does not go on too rapidly, which you can easily do, by dropping the carbonate into the acid very gradually, for otherwise, if hastily and incautiously added, part of the materials would effervesce over and be lost; it is as well, therefore, to guard against this accident by having a large basin, so that if the materials foam up, they may not run over. Well, then, go on adding the carbonate until the effervescence ceases, or, in other words, until the nitric acid is saturated or neutralized by the ammonia. Perhaps if there is any crystalline deposit in the basin during the process, it will be requisite to add an ounce or two of water to dissolve the newly-formed *nitrate of ammonia:* you thus obtain it in solution, and this must be strained or filtered through a piece of fine muslin, tied around the rim of a glass funnel, so that it may bag into its centre; thus filter the solution into a clean earthenware basin, put in a small lump of the carbonate, so as to ensure saturation or neutrality, and set the basin on a ladlefull of sand over the fire, or place it at once in an oven moderately heated, and let it evaporate slowly to dryness; you thus obtain the *nitrate of ammonia* in a solid state, as a white crystalline mass, which you must detach from the basin with a clean knife; put it into a stoppered or well-corked bottle ready for your experiment.

Take a half-pint glass retort, with a long neck, and put into it about two ounces of the nitrate of ammonia; now arrange the retort on a retort-stand, so that the end of the neck dips beneath the water of the pneumatic trough; apply a gentle heat at first to the bottom of the retort, by means of a spirit or argand-lamp, until the nitrate liquefies, then increase the heat, and it will very soon begin to boil. An abundant evolution of gaseous matter takes place; this may be collected in jars, or stoppered bottles; and when you have collected about two or three quarts, you may stop the operation by withdrawing the lamp, and then lifting the neck of the retort out of the water, let it carefully cool, and you will find that a considerable quantity of the nitrate remains in it, which will set into a solid crystalline mass: this will yield more *nitrous oxide* at a future opportunity, and therefore it is as well to keep it in the retort, so that it may always be ready for the production of gas whenever you chance to require it.

Now the theory of the decomposition of the nitrate of ammonia, and consequent production of nitrous oxide, I shall leave until I have brought the nature of *ammonia* before you. We will proceed at once to the examination of the gas collected in the bottles.

The nitrous oxide is gaseous at all common temperatures; by a very strong pressure it passes into the liquid state, so that, strictly speaking, it is not a gas, but a *vapour*.

It supports combustion, and this you can try by plunging a lighted taper into a bottle of it (which you may open in the usual way, because nitrous oxide is heavier than air). The flame of the taper is greatly enlarged, and surrounded by a purplish light.

Now try how it supports the combustion of sulphur; put the sulphur in a deflagrating spoon; hold it over a spirit-lamp till it burns with its *blue* flame, and then plunge it into a bottle of the gas; the flame is *extinguished*. This is contrary to what you would be led to suspect from having seen the combustion of the taper supported. Try if it will support the flame of phosphorus: put a small fragment, about the size of half a pea, into another deflagrating ladle; kindle the phosphorus, and instantly plunge it into a bottle of the gas: it is also extinguished. This is yet more strange than the extinction of the flame of sulphur; but you must not be hasty in drawing the conclusion, that nitrous oxide is incapable of supporting the flame of these two substances.

Try the experiments again, heating the sulphur until it burns with a larger and brighter flame, and then put it into the gas; it will go on burning, almost as brilliantly as in oxygen.

Now let the phosphorus burn a second or two in the air with a bright flame, and then put it into the gas, and it will go on burning with such dazzling splendour, that the eye can hardly bear the light of the combustion: indeed, it is nearly equal to that produced by the combustion of phosphorus in pure oxygen.

But what is the reason that these substances refuse to burn in the one case, but do so vehemently in the other? Why, simply this: when introduced into the gas at the moment that they *first* begin to burn, they are not hot enough to decompose it, and unite with its oxygen; but let them burn a little while in air, and they then get hot enough to decompose the gas, and their combustion is brilliantly supported by its oxygen.

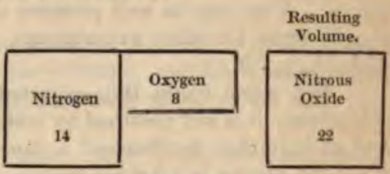
Nitrous oxide is slightly absorbed by water, which takes up about its own bulk, but this absorption does not take place with a degree of rapidity sufficient to interfere with its collection in the pneumatic trough.

It is a little heavier than air, its specific gravity being as 1·534 to 1000, and compared with hydrogen, as 22 to 1.

It is called nitrous *oxide*, because it is not an acid gas; and of this you may presently convince yourself, by putting a bit of litmus paper into a bottle of it, which, if pure, will not affect the test.

It consists of 8 parts by weight of *oxygen*, and 14 parts by weight of *nitrogen*; these, you will remember, are single equivalents of each, the equivalent number of nitrous oxide is therefore 22.

The accompanying symbol may serve to show you the volume and weight in which the gases combine to produce nitrous oxide.



The larger square represents *one volume* of nitrogen, the smaller *half a volume* of oxygen; the weights of these gases are 14 and 8, and they combine to produce *one* volume of nitrous oxide, whose weight, in reference to hydrogen, is equal to 22; so that you see the volume and the half-volume are *condensed* into one volume by combination.

This gas has attracted a great deal of notice, on account of the singular property that it possesses of producing effects resembling those of intoxication when breathed. Sir H. Davy discovered this fact, and he and others very frequently made the experiment of inhaling nitrous oxide; this, however, cannot be done with impunity, for although the effects it produces are, generally speaking, of the most pleasurable and transient nature, yet serious consequences have resulted from inhaling it in large quantities, and in an impure state. I do not advise you to try the experiment, and therefore shall give no directions about the mode of proceeding.

From the excessive degree of involuntary laughter that nitrous oxide generally produces when breathed, it has received the name of *laughing gas.*

We now come to the second compound of nitrogen and oxygen, namely, the *nitric oxide.* The term nitr*ic* oxide is applied to it because it contains more oxygen than the nitr*ous**. Nitric oxide is a gaseous body, permanently so, at all known temperatures and pressures. You may very readily produce it as follows:—take a tubulated retort, about the same size as that used for the production of nitrous oxide, and put into it about an ounce of cuttings of copper, or bits of copper wire,—any old or useless copper will do; then arrange the retort with the beak under the shelf of the pneumatic trough; pour on the copper about half an ounce, by measure, of strong *nitric acid*, previously diluted with an ounce and a half of water; insert the stopper, and if you find no action take place immediately, you must gently warm the bottom of the retort with the flame of a spirit-lamp, but as soon as you find an effervescence taking place, and gas bubbling through the water, remove the lamp, or diminish its flame, lest the materials should boil over: now attentively watch the bubbles of the gas as they burst on the surface of the water, and you will very soon find that they produce a dark orange-coloured vapour at the moment that they break in the air. When this happens, begin to collect the gas in jars or bottles, and when you have got two or three quarts of it, you have enough for the experiments about to be detailed.

If you find the action of the acid on the copper ceases before you have collected the above quantity of gas, (and very likely it will,) you must empty the blue solution out of the retort, (which is a solution of *nitrate of copper*,) and pour on some fresh diluted acid, and resume the operation; if the copper all dissolves, put in a few shreds more, and thus, by adding fresh acid and fresh copper, you may obtain as much gas as you please. You may as well preserve the solution of nitrate of copper, for it will serve for other experiments, as, for example, that mentioned in Vol. II., p. 50.

The nitric oxide thus collected in the bottles is transparent and colourless; it is not absorbed by water; it is instantly fatal to animal life, and extinguishes the flame of a taper. Open a bottle of it in the usual way, and plunge in a lighted taper; it will not burn, but the instant that the bottle is opened, you will observe a dark orange-coloured vapour pervade its interior: this is due to the union of the nitric oxide with the oxygen of the air, forming *nitrous acid*, as we shall presently see; and when I directed you just now to watch for the same orange-coloured vapours attendant on the gas bubbles in the pneumatic trough, it was that you might be certain that nitric oxide was evolving in a tolerably pure state. The production of this orange-coloured vapour is a strong characteristic of the gas in question, and distinguishes it from all other gases.

Now try if it will support the combustion of sulphur, exactly as directed for nitrous oxide, and you will find that it is extinguished; but phosphorus will burn in it with extreme splendour, if you introduce it burning *intensely*, although it is extinguished if immersed in the gas at the first moment of combustion.

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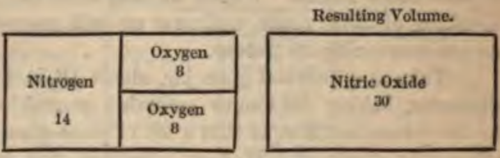
* See Vol. I., page 298.

When phosphorus thus burns in nitric oxide, it combines with the oxygen, forming *phosphoric acid*, and nitrogen is set at liberty; and from the analytical experiments of chemists it would appear, that nitric oxide is constituted of 14 parts by weight of *nitrogen*, and 16 parts by weight of oxygen (so that it contains exactly *twice* as much oxygen as nitrous oxide); its equivalent number is therefore 30.

Such being the case, I think it must be evident to you that nitric oxide consists of *equal volumes* of *nitrogen* and *oxygen*, whose weights are to each other as 14 to 16, as shown in the accompanying symbol, and these gases are combined without any change of volume, or, in other words, 1 volume of *nitrogen* = 14, and 1 volume of oxygen = 16, combine to produce 2 volumes of *nitric oxide* = 30.



Well, then, what is the specific gravity of this gas, as compared with a volume of *hydrogen* = 1? Why, of course, it must be the half of 30 = 15; its specific gravity, as compared with air, is as 1·038 to 1000.

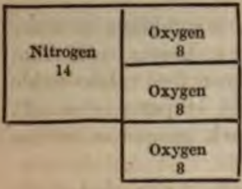
When nitric oxide is perfectly pure, it has no acid properties. Let some of it stand over water to absorb any nitric acid that may happen to contaminate it; then transfer the gas into some clean water, and whilst the mouth of the bottle remains under water, take a little roll of litmus paper, pass it through the water into the bottle, and you will find that the blue colour is not affected; but now withdraw the bottle from the water, so as to let air come into contact with the nitric oxide, the paper is instantly reddened by the sudden formation of the fumes of *nitrous acid*.

Take a tall glass jar, holding about a pint; fill it with water, and invert it on the shelf of the pneumatic trough, and fill it about half-full of *chlorine*; then fill the remaining half with *nitric oxide*, and you get the orange-coloured fumes evolved.

But chlorine contains no oxygen,—how, then, are they produced? Simply thus: *water* is present, and it furnishes *oxygen* to produce the fumes of *nitrous acid*, whilst it also furnishes *hydrogen* to combine with the *chlorine*, and produce *muriatic acid*.

This decomposition of water taking place when chlorine and nitric oxide are mixed together in its presence, was for a long time overlooked by experimenters, who imagined that the production of the orange-coloured fumes was a manifest proof of *chlorine* containing *oxygen*. We now, however, are fully aware of the elementary nature of chlorine; and that it contains no oxygen is satisfactorily determined by a multitude of experiments; and if *chlorine* and *nitric oxide* be mixed together *perfectly dry*, no orange-coloured vapours are produced.

The third compound of *nitrogen* and *oxygen* is called the hypo-nitrous *acid*; the term implying a compound of the two elements intermediate between *nitric oxide* and *nitrous acid*, constituted, that is, of 1 of nitrogen = 14, and three of oxygen = 24; this acid, however, cannot be obtained in its insulated or free state,[2] and is only known in combination with bases, forming a class of compounds called *hypo-nitrites*, which do not possess sufficient interest for these popular essays, and therefore I say no more about *hypo-nitrous acid* than advert to its composition by *weight* and *volume*, as here subjoined.



The fourth compound of *nitrogen* and *oxygen*, viz., *nitrous acid*, will require more notice. I have already adverted to its production, when speaking of *nitric oxide*, and now we will proceed to make some experiments concerning its nature.

Take a cylindrical glass jar, about 18 inches long, and 3 inches in diameter, having its mouth smoothly ground*; apply a little pomatum to the ground surface, so that a bit of plate-glass laid upon it lies flat, and effectually closes it. Having arranged this matter, remove the glass plate, and fill the jar *entirely* full with a very weak infusion of litmus; this will be a pale blue colour; now slide on the glass plate, holding it firmly in its place, and invert the jar on the shelf of the pneumatic trough, but do not withdraw the plate; let the jar stand on it, for the object is to prevent the blue liquid from mingling with the water of the trough.

Now take a bottle of pure *oxygen;* remove its stopper under the water, and let the bottle stand mouth downwards on the shelf, and do the same with a larger bottle of *nitric oxide*, so that they may be both ready in a moment to transfer into the tall jar with the blue liquid; slide it from off the plate on to the edge of the shelf, and quickly transfer into it the oxygen until it is about one-third full; then slide it on to the plate, and leave it for a few minutes: no alteration of colour takes place in the blue liquid, proving that the oxygen is not acid. Now slide it off the plate again as before, and transfer into it about twice as much of *nitric oxide* as *oxygen;* the moment that this is done, the interior of the jar becomes filled with intensely dark orange-coloured vapours of *nitrous acid*, which is thus suddenly formed by the union of the two gases; these are rapidly absorbed by the water, which will mount upwards in the jar; and that an acid solution is thus formed is evident by the reddening of the blue test-liquor. Let this absorption go on as long as it will, and very probably you will find a portion of gas still remaining unabsorbed; now this may be an excess of oxygen, or an excess of nitric oxide; and in order to determine if it is the former, let up a single bubble of nitric oxide; if it produces the orange-coloured fume, add more until this ceases, when you will find absorption has gone on again, and the liquor nearly fills the jar.

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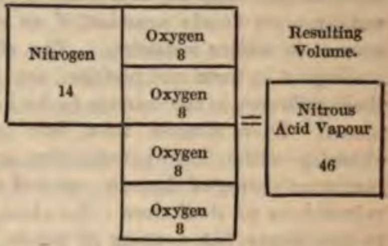
* If you cannot purchase it with this ready done, you can very easily effect it as follows:—on a smooth and flat flag-stone sprinkle some fine "house sand," or emery powder; moisten it with a little water, and then, holding the jar firmly in both hands, apply its mouth to the surface of the stone, and grind it steadily round and round (just as you see a painter *grinding colours*), until you have made the mouth quite flat; you must not apply too much pressure whilst thus grinding the glass, nor must you let it shake about between the hands; and take care, likewise, that there are no lumps in the sand or emery, that the glass can trip over, for if you do not look to all these matters, the glass will very likely "fly;" but work steadily and cautiously, and you are sure to succeed in the operation.

If no orange-coloured vapour is produced, you may infer that the nitric oxide is in excess; and by letting up bubbles of oxygen, cause it to form *nitrous acid*, and the blue liquor to rise in the jar, but very likely it will not entirely fill the jar; and the remaining small portion of gas which remains unabsorbed is due to impurities in the gases employed. However, the combination of oxygen with nitric oxide, and the absorption that has taken place, is quite sufficient to illustrate the formation of nitrous acid.

Nitrous acid consists of 14 nitrogen + 32 oxygen = 46, so that in fact it contains exactly two proportionals more oxygen than nitric oxide, as will be evident from the accompanying symbol. The 1 volume of nitrogen and the 2 volumes of oxygen are, by combination, condensed into the space of 1 volume; the specific gravity, therefore, of the vapour of nitrous acid, as compared with hydrogen, is as 46 to 1.



Now there are methods of obtaining the vapour of nitrous acid, free and independent of water, but these are too difficult for you to manage. I will give you a notion of the mode of proceeding. A dry glass flask, with a glass stop-cock, is exhausted of its air by the air-pump, and then 1 volume of oxygen is transferred into it, and afterwards 2 volumes of nitric oxide; the production of nitrous acid vapour instantly takes place, and as the vessels are perfectly dry, the vapour remains permanent; it supports the combustion of phosphorus very powerfully; if, however, the glass flask be artificially cooled down to the temperature of 0°, the vapour is no longer permanent, but *condenses* into a dark orange-coloured *liquid*, which is *anhydrous nitrous acid*.

This experiment cannot be managed without a good air-pump, and other expensive apparatus; but there is another method of obtaining the same result, of which I shall speak at a future opportunity.

The most important combination of *nitrogen with oxygen* is the *nitric acid;* this will demand a very large share of our attention, and therefore I shall enter most fully upon its history and properties in the next essay.

ON THE INTERNAL HEAT OF THE GLOBE, AND ITS INFLUENCE ON CLIMATES.

[Continued from p. 90.]

We have now freed the problem of terrestrial temperature, as far, at least, as regards the phenomena which take place at the surface, from many of those elements which would have greatly complicated it. Thus it has been shown, that central heat can no longer occasion any perceptible variation in climate, since its *total* effect at the surface does not now exceed $\frac{1}{30}$ of a degree. Whatever doubts may be entertained as to the temperature of space, as estimated by M. Fourier, still that temperature must remain nearly constant, if, as everything induces us to believe, its source is stellar radiation. The changes which the terrestrial orbit undergoes in form and position, are either mathematically inoperative, or their influence is too minute to be indicated by the most delicate instruments. There remain, then, but two causes to account for change in climate,—either local peculiarities, or some alteration in the calorific and luminous power of the sun; and of these, the latter must be immediately rejected, as we shall show: the changes can therefore only be attributed to agriculture, the clearing of woods in plains and on mountains, or to the draining of marshes, &c., if we can succeed in proving that climate has neither become hotter nor colder, in those spots the physical aspect of which has not sensibly varied in a long course of centuries.

The mean temperature of Palestine does not appear to have varied since the time of Moses.

There is a certain mean temperature *below* which the date-palm will not fructify, or rather, the fruit will not ripen. On the other hand, there is another equally determinate degree, above which the mean temperature must not rise, to admit of the vine being profitably cultivated, and of wine being made from the grapes. Now the thermometrical *limits* in opposite directions of these two plants differ but little; if, therefore, we find that at two different distant epochs, the date and the vine ripened *simultaneously* on a given spot, we may affirm that, in the interval, the climate there has not perceptibly varied. Let us now apply these remarks.

The city of Jericho was called the *City of Palms.* The Bible makes mention of the palm-trees of Deborah, situated between Ramah and Bethel, and of those which bordered the Jordan, &c. The Jews ate dates, and prepared them as dried fruits; they also extracted thence a kind of sugar, and a fermented liquor: the Hebrew coins bear distinct representations of date-palms, *covered with fruit.* Pliny, Theophrastus, Tacitus, Josephus, Strabo, &c., all mention woods of palms, situated in Palestine; there can be, therefore, no doubt that this tree was extensively cultivated by the Jews.

We shall find analogous evidence regarding the vine, from which we shall gather, that it was cultivated not only for the sake of its fruit, which was eaten, but in order to obtain wine from the grapes. Every one must recollect the bunch the messengers of Moses gathered in Canaan, which, from its magnitude, required two men to carry it. Allusion is made to the vineyards of Palestine in numerous passages of the Bible: the feast of Tabernacles was celebrated at the close of the vintage; the wines of Judah are mentioned in Genesis, and we further know that the vine was not alone cultivated in the northern and mountainous parts of the country, since the Bible frequently speaks of the wine and vines of the valley of Engaddi: if necessary, I could cite the testimony of Strabo and Diodorus, for both celebrate the vines of Judæa, and I could add that the grape figured quite as frequently as the palm on the Hebrew coins.

In short, it is clearly proved that, in the most ancient times, the palm and the vine were *simultaneously* cultivated in the central valleys of Palestine. Let us now examine what degrees of heat the date and the grape require to arrive at maturity.

At *Palermo*, the mean temperature of which exceeds 17° C. (63° F.), the date-palm grows, but does not ripen its fruit.

At *Catania*, with a warmer temperature, of from 18° to 19° C. (64° to 66° F.), dates are not eatable.

At *Algiers*, the mean temperature of which place is about 21° (70° F.), dates ripen well; nevertheless they are decidedly better in the interior of the country.

From these data we may affirm, that at any epoch when the date-palm was cultivated in the environs, and when the fruit served as food for the population, the mean temperature of Jerusalem could not have been *below* that of Algiers, that is, must have been upwards of 21° (70° F.)

M. Leopold von Buch places the southern limit of the vine at the island of Ferro, in the Canaries, the mean temperature of which is also between 70° and 72° F.

At *Cairo*, and in its environs, where the temperature is 72° F., a vine-stock is here and there seen in a garden, but no vine properly so called.

At *Abusheer*, in Persia, the mean temperature certainly not exceeding 73° F. according to Niebuhr, the vine can only be cultivated in trenches, or in places sheltered from the direct action of the solar rays.

Now we have seen that at the remotest ages in Palestine, on the contrary, the vine was extensively cultivated; the mean temperature could not, therefore, *exceed* 22° C. (72° F.), while the fact of the cultivation of the date-palm has just proved to us that it could not be *below* 70°. We are therefore led to conclude, from the simple phenomena of vegetation, that 21·5 (71° F.) was the degree of mean temperature in Palestine in the time of Moses, with a certainty that this estimate cannot err more than a degree either way.

There are, unfortunately, no direct observations to enable us to form any conclusion as to the mean temperature of that country at the present day; but we can, in some measure, supply the deficiency by terms of comparison taken from Egypt. The mean temperature of Cairo is 22° C. (72° F.), Jerusalem lies 2° northward, and two degrees of latitude in those climates corresponds to a variation of from half to three quarters of a degree of the centigrade thermometer (·9 to 1°·3 F.), which would make the mean temperature of that city rather above 21° (70° F.). Hence, then, we may conclude that there has been no appreciable variation in the climate of Palestine in 3300 years.

But several other agricultural facts concur with the foregoing in indicating the constancy of the climate of this country. The cultivation of wheat, for example, proves that the mean temperature did not *exceed* 24° to 25° C. (75° to 77° F.). The palm-trees of Jericho, on the other hand, would indicate 21° to 22° (70° to 73°F.) as its inferior limit. The Jews celebrated the feast of *Tabernacles*, or that of *vintages*, in October, and it is at the end of September, or towards the commencement of October, that grapes are gathered at the present day in the neighbourhood of Jerusalem.

Anciently the harvest-time in Palestine was from the middle of April to the end of May; and modern travellers have noticed barley in the southern parts of the country, perfectly yellow by the middle of April: near to Acre it is ripe on the 13th of May. And we also know that in Egypt, where the temperature is higher, barley is cut at the end of April or the beginning of May.

It must be observed, that Palestine is one of the countries of the old continent, which must have least experienced those particular modifications of climate, the cause of which we attribute to the agricultural labours of man. Thus the constancy of temperature in this country must lead to this conclusion, that thirty-three centuries have not caused any change in the calorific or luminous properties of the sun. Now the demonstration of this proposition could not be supported by too many proofs, since it has been before stated that the light of some stars, or, as I ought to call them, of some remote suns, has diminished, and even finally disappeared.

On the Climate of Europe in ancient times.

The information as to the agricultural productions of Palestine which we meet with in the earliest writers, is perfectly consistent and clear, the plants are distinctly defined, and their localities exactly designated; hence the conclusions at which we have arrived seem to possess all the certainty that could be expected. It might be hence believed, that this mode of investigation would have equally enlightened us on the ancient climate of Egypt. But this is far from being the case, not that the analogous data are wanting, but because their discordance prevents our drawing any useful conclusion from them. Thus, for example, as regards the vine, Herodotus in one passage tells us that the Egyptians did not cultivate it, while Athenæus celebrates the wines of Alexandria. If we were desirous of ascertaining the southern limits of the culture of this plant, we should find, in the works of Theophrastus, express mention of vines growing near to Elephantina; yet this information would be useless, for the question regarding climate is decided not by the latitude where the vine *vegetates*, but by that at which it ceases to mature its produce sufficiently to furnish wine. The evidence relating to date-palms is not more consistent; according to Strabo, these trees were sterile, or, at least, did not produce eatable fruit, at Alexandria and near the Delta: why, then, was all Lower Egypt covered with them? Let us, therefore, abandon these citations, only calculated to mislead us, founded as they often are upon mere hearsay, and let us proceed to the investigation of our own climate.

I wish it to be again distinctly understood, that it is only local changes that we are about to inquire into, without intending to extend even to a whole kingdom that which we have only determined for particular localities; we should otherwise be violating that spirit of strict induction which is now justly expected in all scientific discussions. It will be advisable previously to examine the endeavours which Daines Barrington, the Abbé Mann, and many others, have made, to prove that the climates of all Europe, and parts of Asia, have considerably deteriorated in the lapse of centuries; and, like these authors, we will begin by citing the cases constituting exceptions, or extraordinary phenomena.

Diodorus Siculus is quoted to show that *the rivers of ancient Gaul* often froze during the winter, and that horse and foot soldiers, chariots, and the heaviest equipages, traversed them without any risk.

The celebrated bridge of Trajan over the Danube was intended, according to Dion Cassius, to facilitate the passage of that river, *when the frost had not congealed its waters.* Herodian tells us that the soldiers on the banks of the Rhine, instead of providing themselves with pitchers to draw water, used to take axes to cut the ice, which they conveyed in pieces to the camp, &c.

Now nothing more is to be drawn from these passages, except that, in the time of the Romans, the rivers of France, the Rhine, and the Danube, were sometimes completely frozen; and from the following table we learn, that at times long subsequent, not only these same rivers, but the Po, the Adriatic, and even the Mediterranean itself, were frequently frozen.

A. D.

860. The Adriatic and the Rhone froze. [The complete congelation of the Rhone near to Arles, or in any other part of Provence, seems, from some observations made in 1776, to imply a degree of cold equivalent to − 18° C. (0° F.) at least. In 1709, when the Gulf of Venice froze, the thermometers in that city descended to − 20° C. (− 4° F.) *Acad. des Sciences*, 1749.]

1133. The Po was frozen from Cremona to the sea. The Rhone was traversed on the ice, and wine froze in the cellars (− 18° = 0° F. at least).

1234. The Po and the Rhone froze; loaded wagons traversed the Adriatic in front of Venice (− 20° = − 4° F. at least).

1236. The Danube remained frozen for the whole length of its course, during a considerable time.

1290. Loaded wagons traversed the Rhine on the ice opposite Breysach. The Cattegat was also entirely frozen.

1305. The Rhone and all the other rivers in France were frozen (*Papon's History of Provence*).

1323. The Rhone froze; travellers on foot and horseback passed on the ice from Denmark to Lubeck and Dantzic.

1364. The Rhone froze to a considerable depth at Arles, and loaded wagons crossed on the ice (− 18° C.). (*Villani*, quoted by *Papon.*)

1408. The Danube froze for the whole of its course; the ice extended

A.D.

uninterruptedly from Norway to Denmark. Carriages crossed the Seine on the ice (*Felibien's Description of Paris*).

1460. The Danube remained frozen during two months; the Rhone also froze (– 18° C.).

1468. Wine was divided among the soldiers by *an axe* (*Phil. de Comines*).

1493. The port of Genoa was frozen up on the 25th and 26th December (*Papon*).

1507. The port of Marseilles frozen all over (indicating a cold of at least – 18° C.) Three feet of snow fell in that city on the 6th January (*Papon*).

1544. Wine in France was cut with edged tools (*Mezeray*).

1594. The sea froze at Marseilles and at Venice (– 20° C. = – 4° F. at least).

1621-22. The Venetian fleet was ice-bound in the Lagunes (– 20° C. = – 4° F.).

1638. The sea froze round the galleys at Marseilles (– 20° C.) (*Papon.*)

1655-56. The Seine was frozen from the 8th to the 18th December, and again uninterruptedly from the 29th to the 18th January. A new frost began a few days afterwards, and lasted till March (*Boulliand*).

1657-58. Uninterrupted frost at Paris from the 24th December to the 8th February. At one time the Seine was frozen solid (*Boulliand*).

It was in 1658 that Charles the Tenth of Sweden crossed the Little Belt on the ice with all his army and its artillery, baggage, &c., &c.

1662-63. The frost lasted at Paris from the 5th December to the 8th March (*Boulliand*).

1676-77. Continued intense frost from the 2nd December to the 13th January. The Seine remained frozen during thirty-five consecutive days (*Boulliand*).

1684. The Thames froze at London eleven inches thick, and loaded wagons passed over it.

1709. The Adriatic, and the Mediterranean at Genoa, at Marseilles, Cette, &c. were frozen (– 18° C.)

1716. The Thames froze at London, and a fair was held on the ice.

1726. Sledges passed from Copenhagen to Sweden.

1740. The Seine frozen the length of its course (– 14° = – 6° F.)
1744. " " " (– 9° = 13° F.)
1762. " " " (– 9°.)
1767. " " " (– 16° = – 8° F.)
1776. " " " (– 12° = – 11° F.)
1788. " " " (– 13° = – 10° F.)
1829. " " " (– 14·5° = – 9° F.)

I much doubt if any one, after studying this table, can find, in the phenomena of the congelation of rivers mentioned by classical writers, *any proof* that the climate of Europe has deteriorated.

Virgil (*Georg.* B. III.) advises that straw and heath should be strewed in the pen-fold under the lambs, lest the cold should injure these delicate animals; and there are authors who seriously quote this vague and trivial passage as an irrefragable proof that the winters of ancient Italy were more rigorous than we can form any idea of. I have already replied to these exaggerations, by showing that, in *modern* Italy, the Po and the Adriatic are frequently frozen; but if anything more is required, I may state that at Padua, not far from Mantua, the birthplace of Virgil, there fell in 1604 so great a quantity of snow, that the roofs of many of the principal houses were crushed in by the weight, and *the wine froze in the cellars of the city*, &c. How can the straw litters recommended by the author of the Georgics, be put in competition as meteorological documents with such well-attested facts?

The same poet somewhere states that the rivers, even in Calabria, occasionally froze; what can be opposed, it will be said, to such a phenomenon; how can it be denied, after this, that the winters in the south of Italy were formerly much colder than they are at present? The objection, however, has not so much weight as might at first be imagined. I may, in the first instance, remark that the occasional congelation of a river cannot characterize a climate; that diverse atmospherical circumstances may combine to precipitate on *any* spot of the globe the dry cold strata from the elevated regions of the air; that the low temperature of these strata, the cold resulting from the abundant evaporation which their aridity gives rise to, added to that which always ensues during tranquil, serene, cloudless nights, from the radiation of heat into space, appear, when united, sufficient to occasion the congelation of a river in all regions of the world. Thus, not many years ago, the water in the skins carried in Captain Clapperton's caravan, froze near Mourzouk, one night, in a plain but little elevated above the level of the sea, and the assertion, made by Abd-allatif (see the French translation by M. Silv. de Sacy, p. 505,) that in 829, when the patriarch of Antioch went with the Caliph Mahmoud into Egypt, *they found the Nile frozen*, is not considered by meteorologists as unworthy of credence, or as inexplicable.

A perfectly distinct passage in Theophrastus asserts that in ancient times the dwarf-palm (*Chamærops humilis*) covered large tracts of country in Calabria. Now this plant can vegetate, it is true, in a country subject to accidental frosts of short duration, as in the province of Valencia, but frequent or protracted ones, capable of freezing rivers, would inevitably destroy it. Strabo relates (Book II.) that the frosts are so severe at the mouths of the Palus Mæotis, that in winter one of the generals of Mithridates defeated the barbarian cavalry on the precise spot where, in summer, they had been vanquished in a naval combat. This is one of the passages that has been most frequently quoted by the advocates of the theory of general change of climates. Pallas, who sojourned a long time in Southern Russia, says that, even during the ordinary winters, the masses of ice brought down by the Don are deposited at, and cover over the strait of Zabache, and a large part of the sea of Azoff; and that in severe seasons loaded wagons cross without difficulty over the ice from one shore to the other.

Of the Climate of the environs of Rome.

Theophrastus and Pliny relate that the plains in the environs of Rome were covered with beech-trees; the highest mean temperature compatible with the thriving of this tree does not exceed 10° C. (50° F.); the mean temperature of Rome is now about 15½ C. (60° F.) If there be no error in the two authors I have cited, as to the designation of the species of tree, nor of the localities, if it is of the plains, and not of the mountains, they intended to speak, the ancient climate of Rome must have been singularly ameliorated in the course of ages; to a temperature a little below that of Paris would have succeeded the temperature of Perpignan! But the suspicion that there must have crept some error into the passages alluded to, is confirmed by this—that the second of the two authors above cited, after having spoken of the beech, states that the laurel (*Laurus nobilis*, the bay tree) and the myrtle also grew in the plain of Rome; now this would require a mean temperature of 13° or 14° C. *at least*[*] (56° to 58° F.)

Though these are only *lower* limits, they are not very different from the actual temperature; and let us add, that, in the time of Pliny, the laurel and myrtle, as he informs us, prospered in the middle of Italy, *even to some height up the sides of the mountains.* Finally, since, according to the testimony of all travellers, these plants are not found in such situations at a height exceeding 403 yards, we may conclude, without hesitation, that ancient was certainly not *colder* than modern Rome.

A passage in Pliny the younger seems to me to prove also that it was not *warmer;* in his epistle to Apollinarus (B. 5, l. 6), he says, when speaking of a field in Tuscany, "Laurels grow there, and if they sometimes die, it is not oftener than they do in the neighbourhood of Rome." Thus it appears, laurels sometimes perished in the neighbourhood of Rome; the ordinary mean temperature of that city, therefore, was but little above that which is fatal to the laurel—that is, could not much exceed 13° C. (55° F.) The habitual vegetation of the laurel and myrtle indicates 14° (57° F.), while the occasional death of the former gives us a number but little exceeding 13° C.; and thus we are led to the conviction that Rome has ever possessed a constant mean temperature, for at present it is, as I have before stated, 15·5° (59° F).

Varro places the vintage between the 21st September and the 23rd October. On an average, the 2nd October is the date at the present day, which confirms the conclusions deduced from the culture of the laurel and myrtle; and there are two remarkable passages from Virgil and the elder Pliny, which will still further confirm the opinion that the plains of Romagna were not so cold in ancient times as some writers have asserted. On ascending the Apennines to a certain height above the level of the sea, a great number of beautiful trees are met with, which could not support the high temperature of the plains; among them I will only cite the common fir, and the pitch pine *(Pinus picea)*; now the above authors mention this very fact, and state that the *high mountains* were the only localities in which these trees were met with.

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[*] These limits of temperature are only true for continental climates; in islands, those especially where almost constant westerly winds coming from the sea considerably moderate the winters, the myrtle can live in *mean* temperatures far below 13° C. (55° F.) This plant thrives admirably on the coast of Glenarm in Ireland in 55° N.; but then it rarely freezes *there,* the winter being much milder than in Italy. But what is gained during the cold season in such localities is lost with usury in summer; thus grapes do not ripen on the coast of Glenarm. I refer all those who are desirous of studying thoroughly the differences between continental and maritime regions, as to the distribution of the same amount of annual heat among the seasons, to M. Humboldt's *Memoir on Isothermal Lines.*

It must be confessed that the data which have been employed in this discussion are deficient in the precision of those which furnished us for Palestine with two limits of temperature nearly identical; but we need the less regret this, since if we had been led to attribute a difference of temperature for Rome of even two or three degrees, we could not have assigned any cause, for want of knowing with certainty what the ancient state of the country was, compared with its present.

Change of Climate in Tuscany.

Pliny the younger, in the letter to Apollinarus already alluded to, states that the climate of his estate in Tuscany did not suit either the myrtle or the olive; and yet the villa was not situated on an eminence; he expressly says that it was near the Apennine, at the foot of a hill, not far from the Tiber. It is for the inhabitants of *Città di Castello*, the ancient Tisernum, to decide if, as I believe it to be, the climate is at present more moderate than in the time of Pliny; at all events, it would be well to inquire if the surrounding mountains are still covered *with trees very old and very lofty*. But let us proceed to consider modern Tuscany.

As soon as Galileo had invented the thermometer, towards the end of the sixteenth century, the academicians *del Cimento* caused a great number to be constructed, perfectly alike, and distributed them in different cities of Italy, that they might be employed in simultaneous meteorological observations. At the same time, the Grand Duke of Tuscany, Ferdinand II., directed the monks of the principal convents situated in his states to engage themselves in these interesting labours. The enormous mass of documents which were collected in consequence of these proceedings, were dispersed at the time when Prince Leopold de Medicis sacrificed the academy *del Cimento* to the resentment of the court of Rome, in order to obtain the cardinal's hat.

A few volumes only, by a miracle, escaped the vandalism of the Inquisition, among which are to be found part of the thermometrical observations made by Father Raineri at the convent *dei Angeli* at Florence; these observations, compared with those of modern meteorologists, seem, on account of their antiquity, calculated to throw some light on the question of climate; unfortunately the thermometers of the academicians *del Cimento* had no fixed point, and the endeavours which have been made to compare the degrees of these instruments with the centigrade, and with Fahrenheit's scales, have not been very satisfactory · this was the state of the case when, in 1828, a chest was discovered at Florence, containing, among many other old instruments, a great number of thermometers of the *Cimento*, divided into fifty parts. M. G. Libri, in whose hands they were placed, and they could not have been better placed, began by examining whether they agreed with one another as to their graduation; he afterwards, by means of more than two hundred observations of comparisons, referred their scale to those now in use. Thus, for example, M. Libri found that the zero on the *Cimento* scale corresponded with – 15° of Réaumur; that the 50° of the former is identical with the 44° of the latter, and that the thermometer of *Cimento* plunged in melting ice marked 13·5°, &c.

Furnished with these results, M. Libri, from the recovered registers of P. Raineri, which embrace a period of sixteen years, has deduced and tabulated the *maxima* and *minima* of each month of that epoch, with the analogous observations made since 1820 at an observatory at Florence. This table indicates this important conclusion, that the clearing of the woods from the mountains, which has been effected since the last sixty years, contrary to a generally-received opinion, does not appear to have produced any perceptible diminution of temperature in Tuscany. In the sixteenth century the Apennines were covered with forests, and yet, between 1655 and 1670, P. Raineri saw his thermometer one year at – 5° C. (23° F.), another at – 5°·6 C., during a third at – 9°·4 C. (15°F.), and during a fourth at – 13° (8° F.),—degrees of intense cold, which were not attained in the extraordinary winter of 1829-30.

The column of *maxima* of temperature in M. Libri's table seems to me to present another inference as important; it clearly results from it, in my opinion, that in the sixteenth century the summers were *warmer* in Tuscany than they are now. The observations of Raineri present five maxima of 37½° C. (99·5° F.), two of 38½° (101·5° F.), and one of 38·7° (102° F.). From 1821 to 1830 the thermometer at Florence only rose to 37½° C. once. Thus *milder winters and colder summers* appear to be the result of the modifications which the climate of Tuscany has undergone.

Changes in the Climate of France.

The documents which I am about to lay before the reader appear to me to prove, that in certain regions of France *the summers at present are less warm than they formerly were.*

Many ancient families of the *Vivarais* have preserved among their charters, documents remounting to 1561, which allude to the existence of productive vineyards in districts elevated more than 600 yards above the level of the sea, and where at present not a single grape ever ripens, even in the most favourable situations. It must be conceded, therefore, that the summers in the Vivarais were formerly hotter; and this conclusion is confirmed as regards a part of the country where the vine is still cultivated, by a document equally authentic, but of a different kind.

Before the revolution, there were in the *Vivarais* a great number of quit-rents, *established in the sixteenth century, to be paid in wine;* the greatest number of these were to be paid in the wine first drawn off from the vat; and it was stipulated of others, that the wine should be taken *in the tuns* at the choice of the landlord, the term of payment being fixed for the 8th of October (allowance being made for the change in the calendar). These deeds prove, therefore, that the wine was in the *tuns*, or at least in the vat, by the 8th of October, and ready to be drawn off. Now eight days is the minimum of time for which the wine is left in the vat before it is fit to draw off; in the sixteenth century, therefore, in the Vivarais the vintage must have been finished by the last days of September; at present it is not effected till between the 8th and the 20th of October; an inhabitant states that he has never seen it *begin* before the 4th. These documents are silent as to the duration and rigour of the winters; but I repeat they appear to establish the fact that at the 45° of N. latitude, on the banks of the Rhone, in the sixteenth century, the summers were warmer than they are at present.

We read in the history of *Mâcon*, that in 1552 or 1553 the Huguenots retreated to Lancié (a village situated near to that city), and that they drank the *Muscat wine of the country;* at present, the Muscat grape does not ripen sufficiently in the *Mâconnais* to admit of wine being made from it.

The Emperor Julian caused the wine of Surêne to be served at his table: the reputation of the *vin de Surêne* is at present proverbial, but certainly not for its excellence; not that I attach undue importance to this comparison,—the *quality* of wine depends too much on the nature of the plant, and on the cares of the cultivator, to furnish irrefragable arguments on a question of change of climate. We find in an old charta quoted by M. Capefigue, that Philip Augustus being desirous of selecting one for his habitual drink from the European wines, the vineyards of Etampes and of *Beauvais* offered themselves among the competitors: the charta adds, it is true, that they were rejected; but can it be imagined that they would have ventured to present themselves if their produce had been as undrinkable as are all the wines of the department de l'Oise in our days? This department now marks the northern limit of the culture of the vine: the account for 1830, furnished by the Administration of Indirect Taxation, in fact states, *that there is no wine-making in the department de la Somme;* now it is not in the districts where the cultivation is hardly possible, that tolerable produce could ever have been obtained.

When the Emperor Probus permitted the Spaniards and Gauls to plant vines, he granted *the same favour* to the English; the *favour* would have been a mockery, if at that time the vine had not fructified on this (English) side of the channel; accordingly, we learn from old chronicles, that at one period the vine was cultivated in the open fields over a great part of England, and that wine was obtained from the produce; now the most assiduous care, a southern situation, completely sheltered from the cold winds, and an espalier, are hardly sufficient to bring a few small bunches to complete maturity. This is enough to convince the most incredulous, that in the course of centuries, the summers, both in France and England, have lost a proportion of their temperature: it remains to seek the cause of so alarming a phenomenon.

This cause cannot, evidently, be in the sun; the constancy of the temperature in Palestine is sufficient to prove that: some *physiciens* imagine it lies in an unusual extension of the ice at the Arctic pole, some general movement of which, after having drawn it by several degrees southward, has accumulated it on the coast of Greenland. It is certain, that the eastern coast, when it was discovered, towards the end of the tenth century, was free from ice: the Norwegians established a colony there, which in 1120 was populous, flourishing, and carried on a considerable commerce with the parent country and with Iceland. We know, also, that in 1408, when Bishop Andrew (the fourteenth since the establishment,) went to take possession of his diocese, he found the coast entirely blockaded by the ice, and not approachable. This state of things lasted, with little variation, till 1813 or 1814, when a great breaking-up of this icy barrier took place, and the eastern coast of Greenland again became clear. The deterioration of the European climates ought then, according to this theory, to have been due to the permanent existence of a large plain of ice, extending in latitude from Cape Farewell to the Arctic circle.

I shall completely overset this explanation, by remarking, that the documents I have brought forward to prove that in the Vivarais and in Burgundy the heat was formerly greater, are of a date *a century and a half posterior to the formation of the Greenlandic plain of ice;* and I shall add, that the nearly entire breaking-up which the ice experienced in 1814 has occasioned no change in our climate that has been revealed by any agricultural phenomenon, nor even any of the more delicate modifications, the existence of which can alone be shown by meteorological instruments. Let us now see if this variation in climate has not resulted from the works and labours, which the wants or caprices of an increasing population have caused to be accomplished over the country.

Ancient France, compared to what it now is, would present us with a vast extent of forest, and mountains almost entirely wooded; with interior lakes, ponds, and marshes, without number; with rivers, the overflow of which was restrained by no embankment, and with extensive tracts never furrowed by the plough, &c. The clearing away entire forests, and the cutting of large openings into others, the almost entire disappearance of stagnant waters, the breaking up of large tracts for agriculture, which differed little from the steppes of Asia or America,—such are the modifications which the surface of France has undergone in an interval of some hundreds of years. Now there is a country which at this very time is undergoing these same modifications, under the eyes of an enlightened population, and where they are proceeding with astonishing rapidity: in the country alluded to they must palpably cause, one after another, those meteorological changes which many centuries have hardly sufficed to render evident on the old continent. Let us then see how the clearing of land in America changes the climate.

Throughout all America it is agreed, that clearing and cultivation have modified the climate, and that the change becomes more and more manifest; *the winters are less severe, and the summers less oppressive:* in other terms, the extremes of temperature observed in January and July approach each other, from year to year. Compare these results with those which flow from the foregoing discussion; as regards Florence, the identity is evident; we see that in the centre and north of France, as in America, the summers are become less hot; perhaps also, according to the prevailing opinion, the winters were formerly colder, but we have not discovered that this great severity of the ancient winters is *proved.* In every case, nothing that we can see contradicts the doctrine, that in

Europe the change of climate ought to be exclusively attributed to the clearing and cultivation of land.

The Americans have also recognised a marked modification in the winds which blow on their coasts (see the works of Williams and of Jefferson). The former predominance of westerly winds appears to diminish[*]; the easterly become more frequent, and also penetrate by degrees further inland. The effect of clearing in the United States has been a diminution of cold and heat; but that does not tell us whether the *mean* temperature there has suffered any alteration; the advantages gained on the milder winters may compensate the loss of heat during the summers. It is probable, however, that this compensation does not take place; for, among the important remarks collected by M. Boussingault during his residence in America, there is a table of observations of mean temperatures of the equatorial zones, in which we find, without exception, the lowest numbers correspond to the wooded regions. It should appear, then, that while the American climate becomes less *excessive*, to use an expression of Buffon, its mean temperature increases.

It will not be useless to add a few words in reply to those *physiciens* who refuse to see in the operations of human industry, in those labours which, it must be owned, hardly affect the mere surface of the globe, a sufficing cause for a perceptible change of climate. I will content myself with pointing out different localities which enjoy an unusual climate, either in consequence of some hill favourably placed relatively to the prevailing winds, or of some undulations in the territory, or of other circumstances not more remarkable.

Middlebourg, the latitude of which is nearly a degree less than that of *Amsterdam*, ought to have a mean temperature higher by half a degree; and yet it has one lower by more than two degrees.

The city of *Brussels* itself has not a mean temperature as high as that of *Amsterdam*, although it is placed one degree and a half to the south of it.

The town of *Salcombe*, in Devonshire, enjoys a climate so extraordinary, that it is termed the *Montpellier of the North*.

Marseilles is more than a degree to the south of *Genoa;* the mean temperature of the latter *ought* therefore to be half a degree, C., lower than that of Marseilles; on the contrary, it is one degree *higher*.

It is not surprising that Marseilles, a maritime city, should have a more temperate climate than Avignon, situated a little more to the north and inland, that the winters should be less cold, and the summers sensibly less warm; but what is the reason that the *mean temperature* of Marseilles is lower than that of Avignon?

Rome and *Perpignan* have exactly the same mean temperature, although Rome is one degree to the southward. If I were to ask the reason, I should be reminded of the Apennines, and yet Perpignan is at the foot of the Pyrenees.

I will not extend these examples. I know that plausible explanations of many of the anomalies they record can be deduced from the configurations of the countries in which the different cities I have mentioned are situated: but the question does not rest there; what I say, and what I still maintain, is, that the presumed causes of these anomalies are but little remarkable, and that an attentive examination of the localities would never have enabled any one to have divined of what nature their influence would be, nor in what direction it would operate.

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[*] If any one doubts of the immense predominance of westerly winds in the Atlantic Ocean, I would cite the following conclusive fact:—By a mean of six years, the packets which proceed from Liverpool to New York take forty days to effect the passage, while they return in twenty-three days.

I now approach the conclusion of my task, and shall terminate this paper by briefly examining if the mean climate of Paris experiences any variation in our days. Nothing appears at first more simple than this question. The temperature of subterraneous places at a little depth, to which the external air has not free access, not only does not vary, but it is equal to the mean temperature of the open air at the surface. There exist such vaults under the Paris Observatory, eighty-six feet below the surface; the thermometer has been observed in them for a century and a half; and it is only necessary to compare the observations.

Without recurring to the more ancient instruments, for their graduation is now not well known, I ought to state that a recent discovery has rendered the solution of the problem difficult. It is now proved, that *all* thermometers become in process of time erroneous; the zero, or term of melting ice, *ascends* along the graduated scale, as if the bulb had contracted; the instrument thus comes to indicate + 1° when it ought to mark zero; and + 2° when it ought to stand only + 1°, and so on; the error is sometimes as much as 1½°. The numerous temperatures determined in the vaults of the Observatory at a period when it was not known that thermometers required frequent verification, are, therefore, in some measure fruitless: I have nevertheless met with two, and only two, from which we may gain some instruction: they date from February, 1776, when Messier made them with a thermometer constructed under his eyes, and verified by himself but a few days before. These two observations agree perfectly, and give 11·8° C. (53·2° F.)

In 1826, half a century afterwards, we also find 11·8° C.

Now, supposing Messier's observations are uncertain by the twentieth of a degree, on account of the smallness of the scale of his thermometer, the two temperatures of 1776 and 1826, which appear identical, *might* differ by that one-twentieth; but one-twentieth in fifty years is one-tenth in a century, and *would only be one degree of variation in a thousand years.*

The two epochs compared comprise between them a period during which certain parts of France have been much stripped of forests, and yet the mean temperature of Paris has not thereby been sensibly affected. I have taken the observations of 1826 as the term of comparison, for the sake of the exact half-century; if I had gone on to 1833, I should have found seven-hundredths of a degree more: thus, instead of a diminution of temperature, we should have had a slight increase indicated to us. But we must wait three or four years more before we can affirm with certainty that the 0·07 which I have just alluded to is more than an irregular and accidental oscillation.

A POPULAR COURSE OF GEOLOGY.

VII.

CHEMICAL CHARACTERS OF MINERALS.—THE BLOWPIPE.

ONE of the greatest difficulties to be encountered by the student in discovering the name of a mineral by means of the external characters, arises from the classification usually adopted in systems of mineralogy being founded, not on those characters, but on the composition of minerals as deduced from analysis. This is, without doubt, the most scientific mode of classification; but at the same time it ought to be accompanied by an artificial division into classes, orders, and genera, founded on the external, or the pyrognostic characters, or on a combination of the two, in order to enable a person unacquainted with the name and composition of a mineral, to ascertain in what part of the scientific system to look for a description of it.

In the absence of assistance from such an artificial arrangement, serving as an index to the philosophical system, the student's only resource, after observing and noting down the external characters of the specimen the name of which he wishes to discover, is to wade through the volume which he consults, until he finds a species of mineral agreeing with his own description of the specimen under examination. The only work known to us in which the external characters are made the basis of any such artificial classification, is *Bakewell's Introduction to Mineralogy*, in which a tabular view is given of the most important earthy minerals, grouped according to their hardness and specific gravity. *Aikin's Manual*, a work now out of print, contains, in like manner, a table of their pyrognostic characters, or habitudes before the blowpipe; and in *Griffin's Treatise on the Use of the Blowpipe*, these two modes of classification are combined in a system founded on the most important physical and chemical characters. This is a cheap and useful little work, which, as a companion to the blowpipe, ought to be in the hands of every one desirous of acquiring a knowledge of mineralogy. Under its guidance, we mastered the use of the blowpipe without the aid of other instruction, and made, in a short time, more progress in identifying minerals than we had made in years while relying on other sources of information; and we have no hesitation in declaring that with this little volume before him, and the blowpipe in his hand, the student possessed of only moderate perseverance will have no difficulty in satisfying himself of the name of any mineral which he may meet with.

In that work will be found a history of the application of the blowpipe to chemical investigations, together with minute directions for manipulation. Our limits will not permit us to enter so much into detail; neither shall we discuss the relative merits of Gahn's, or Tenant's, or Wollaston's blowpipe, or of any of those other modifications of the form of the common instrument which bear the names of the philosophers who have attempted to improve it. One reason for our silence on this point is, that we have had no experience in the use of any blowpipes except the common instrument used by working jewellers, and that called Cronsted's, which has a bulb near its smaller end, designed to obviate the inconveniences arising from the condensed moisture of the breath; and, after a few trials, we gave the preference to the common instrument, which we have since exclusively used.

The student should be provided with two blowpipes of the common form (fig. 20); one with a very fine orifice, for fusing earthy minerals and for reducing ores to the metallic state; the other with an orifice somewhat larger, for roasting ores and oxidizing metals in the outer flame. Besides these, the following apparatus will be required for ascertaining the chemical characters of minerals, either by the dry or the humid process; some of the apparatus being used in the one, some in the other, and some being equally useful for both.

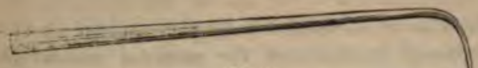


Fig. 20.

Fig. 21 is a pair of forceps, of brass, tipped with platinum, for holding small fragments of earthy minerals in the blowpipe flame.

Fig. 21.

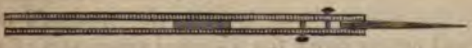
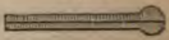


Fig. 22.

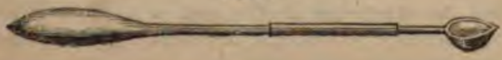


Another pair of forceps will be required, having at the extremity a pair of cups of platinum, (fig. 22,) for holding minerals which decrepitate on the application of heat, and which would fly about and be lost, unless confined until heated to redness, when decrepitation ceases. As a substitute for these latter forceps, a small piece of platinum foil may be used, in which the mineral can be folded up, and thus preserved during decrepitation. Platinum foil, cut into narrow strips about two inches long and half an inch broad, is also useful for supporting some minerals when treated with fluxes; platinum being so bad a conductor of heat, that one extremity of a piece of foil, of the size above mentioned, may be held in the fingers without inconvenience, while the other end is heated to whiteness in the blowpipe flame.

Platinum wire, cut into lengths of two or three inches, and bent into a small hook at one end, is very convenient as a support for a mineral to be heated with a flux. The end of the wire is to be moistened in order to make the flux adhere, which is then fused into a globule with which the *assay*, or mineral to be examined, is mixed by bringing it into contact with the fused globule while still soft, or by moistening its surface when cold. The two substances are then fused together, and an insulated mass is thus obtained, which is easily examined, and which may be detached by gently tapping the wire while hot. Another mode of operating is by mixing the flux in a small mortar with the mineral previously reduced to a fine powder, and then fusing them together. This wire is the most convenient support in those cases where the circumstances to be observed are the changes of colour produced by fusion with borax, soda, or microcosmic salt.

Fig. 23 is a small spoon, the bowl of which is of platinum, having a short shank of the same metal soldered to a longer shank of silver, with a

Fig. 23.



wooden handle. This is a support for earthy minerals, and for such ores as are not reducible to the metallic state without fluxes. It is more easily managed by beginners than the foil or the wire; but as these abstract less of the heat from the assay, they are preferred by adepts in blowpipe manipulations. To them the chief use of the spoon is for fusing a mineral with soda, though, even for that purpose, some prefer charcoal as a support. Minerals in the metallic state, or which are reducible to the metallic state without fluxes, should never be fused on supports of platinum, as they form alloys with it. Minerals containing sulphur and chlorine likewise injure the platinum. For such minerals, charcoal is the proper support. The best kind of charcoal is that made from the wood of the alder. It should be straight and free from knots, and should be cut with a fine-toothed saw into strips from four to six inches long. A small hollow is to be made at one end of the charcoal, with the point of a knife, or other convenient instrument, and in this hollow the assay is to be placed, in order to prevent its being carried off the charcoal by the blast; and the blowpipe flame is then to be directed upon it. Charcoal that is liable to crack should be gently heated before the assay is applied, in order to prevent burning fragments of it from flying about, thus endangering the eyes of the experimenter, as well as causing the loss of the mineral to be operated upon.

Charcoal is used as a support for the fusion of the metallic oxides, because it facilitates their reduction by the abstraction of oxygen during its combustion. It is likewise used as a support for compounds of the metals with sulphur, arsenic, and chlorine. Plates of mica are sometimes used as supports, when, in roasting ores, it is desirable to avoid the reducing effects of the charcoal.

Glass tubes of several kinds will be required for various purposes, both in experiments by the blowpipe and by the humid process. Fig. 24, *d*, is a tube two or three inches long, one-eighth of an inch in diameter, and open at each end, for the purpose of heating minerals which volatilize at a low temperature. The assay is placed at one end, and heated by the blowpipe flame, when volatile matter not permanently gaseous will condense in the upper part of the tube; but a tube open at one end, and blown into a bulb at the other, (fig. 25,) is more commonly used for this purpose, as well as for heating minerals which decrepitate, and for ascertaining the presence of water in them. In this form it is called a matrass, and is very useful for effecting solutions on a small scale, with acids, when the application of heat is necessary.

Fig. 24.

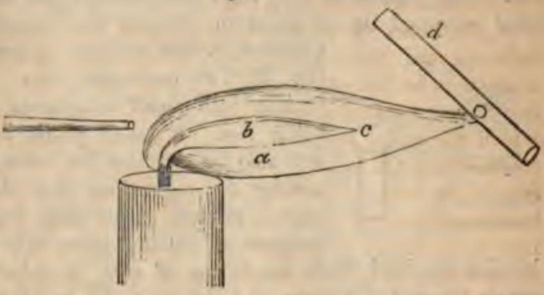
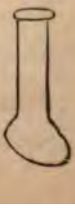
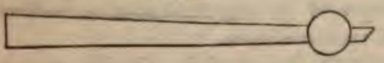


Fig. 25.



Supports of pipe-clay are sometimes used for exhibiting the colours produced by the fusion of borax and the other fluxes with different metals. They may be made in two ways. Either the clay may be beaten out into very thin plates, between two sheets of paper, the clay and paper being then cut together into pieces two or three inches long and a quarter of an inch wide, or small bowls or cups of clay may be prepared in the following manner:—A piece of brass, one-twentieth of an inch thick, is to be pierced with several circular holes, not quite a quarter of an inch in diameter. It is then to be placed on paper, and the holes are be filled with moist pipe-clay, worked to the consistency of dough. The superfluous clay being removed, the brass mould with the paper under it is to be placed in the palm of the left hand, while with a finger of the other the clay in the holes is to be pressed so as to give it a concave form. These cups, as well as the strips of clay before mentioned, having been slowly dried, are to be hardened in the fire in a small crucible or the bowl of a tobacco-pipe. In using one of these cups, it may either be held in the inner flame of the blowpipe with a pair of forceps, or it may be cemented to a thin strip of glass, (fig. 26,) with silicated potassa, for use in the outer flame, or it may be cemented with clay to a thin handle of pipe-clay, and then burned, thus forming a small earthen spoon, which, being white, exhibits, to great advantage, the characteristic colours produced by the metals with fluxes.

Fig. 26.



A small mineralogical hammer, broad at one end, and sharp at the other, will be wanted for chipping off fragments of minerals for examination, and for ascertaining the malleability of a metallic globule after fusion, by flattening it on a plate of steel, called a watchmaker's anvil. This hammer and anvil may likewise be used for crushing ores both before and after they have been roasted, the mineral to be crushed being wrapped up in paper to prevent its dispersion. This process, however, is best effected by means of a steel mortar (fig. 27,) so contrived that a mineral may be broken into small fragments, without any of it being lost. A small pestle and mortar of agate will also be required for reducing a mineral to fine powder for operations with the blowpipe, or with acids. Several small files will be useful for the purpose of cutting glass tubes, and for filing the globules obtained from the fusion of ores, in order to ascertain if they possess the metallic lustre. A knife is also indispensable for trying the hardness and sectility of minerals. Knives for the use of the mineralogist are made to contain also a magnet and a file. The magnetic needle and electrometer have already been described. A lens must not be omitted, for examining the structure of a mineral both before and after it has been operated upon by heat. A pair of strong cutting pliers, for breaking off portions of minerals, will be found very useful, as also a pair of brass forceps,

Fig. 27.

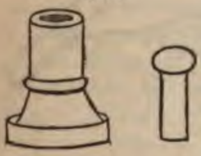
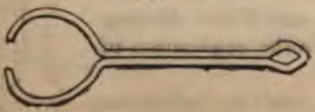


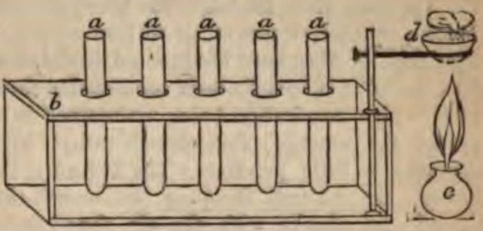
Fig. 28.



for taking up small particles of a mineral to be tried, and for handling globules of reduced metal while hot. To these we may add a small wire bent into the annexed shape (fig. 28,) for holding a watch-glass over the flame of a lamp.

We have already spoken of the open glass tube and matrass. Some of the following apparatus of glass will occasionally be used for operations with the blowpipe, but more frequently when solutions are effected with acids. Large watch-glasses, or the lower part of an oil-flask, are useful for submitting small portions of a mineral to the action of acids, and for evaporating solutions: *a a* (fig. 29) are test-tubes, in which solutions of minerals are to be tried with different tests or re-agents; *b* is the stand for holding them, to which may be attached an apparatus of wire, *d*, for supporting a watch-glass over the flame of a spirit-lamp, *c*. Two or three dozen of these tubes should be provided, of different sizes. The largest need not be more than half an inch in diameter, and four or five inches long. They should have a lip in the brim for the convenience of pouring out fluids.

Fig. 29.



Figures 30, 31 are dropping-tubes, of different kinds. That represented in fig. 30 was the invention of Dr. Wollaston, and consists of a glass tube, with a fine orifice bent at a right angle about an inch from the orifice, and passing through a cork fitted in the neck of a bottle, which is to be about half full of water. The warmth of the hand when the bottle is held in it, dilates the air in the bottle, and slowly expels the water. The other dropping-tube (fig. 31) consists of a narrow glass tube about six inches long, with a bulb about an inch in diameter blown out near the middle, and drawn out at the other extremity into a capillary orifice. This end being immersed in water, the bulb is filled by the action of the mouth on the upper extremity. The water is then prevented from escaping, by closing this orifice with the finger, on the removal of which, the water falls out in drops. These dropping-tubes are used for washing precipitates on filters, and in experiments with the blowpipe are frequently used for separating small fragments of charcoal that have become mixed with particles of reduced metal. A tray will be necessary to catch the assay when it falls from the support. This should be made of tinned iron; it should be about two feet long, and eighteen inches wide, and should be turned up one inch and a half at the edges. It should be lined with white paper, and, to prevent dubious results from the mixture of different substances, should be cleaned every time an experiment is commenced with a fresh mineral.

Fig. 30.

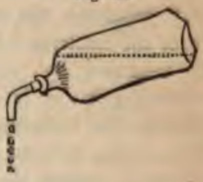
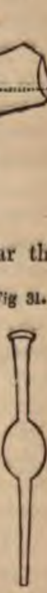


Fig 31.



With respect to the flame employed, that of a candle—an ordinary mould or wax candle—with rather a large wick, is sufficient for the fusion of minerals that are to be held in the forceps, or tried with fluxes on the platinum wire or foil, or in the clay cups. When charcoal is employed as the support, there is this disadvantage attending the use of the candle—that the heat radiating from the charcoal is apt to melt the tallow or wax, and to make it *gutter*, or burn away too fast. In that case, a lamp made of tinned iron is the most convenient; it may be fed with oil, tallow, or hogs' lard. The flame of a spirit-lamp gives a strong heat free from smoke, the only objection to it being the increased expense attending the employment of alcohol as a combustible. When a lamp is used, the wick should be parted in the middle, in order to expose as great a surface as possible to the flame.

The great art in the use of the blowpipe consists in maintaining a continuous, equable stream of air so long as the experiment requires it. To effect this, the blast must not proceed directly from the lungs, but the cheeks must be inflated, and by their compression the air must be forced through the blowpipe, respiration being maintained in the mean time by breathing through the nostrils; this, though rather difficult at first, will become easy after a little practice. The beginner should first learn to breathe through the nostrils, keeping the mouth shut. Let him then learn to distend the cheeks with the air thus inspired, and to make several respirations, without suffering any air to escape from his mouth; when able to accomplish this, let him take a blowpipe between his lips, and having filled his mouth with air, let him expel it gently through the tube by the action of the muscles of the cheeks, while he breathes through the nostrils. To this end the tongue must be applied to the palate, so as to interrupt the communication between the mouth and the passage from the nostrils. As the supply of air in the mouth diminishes, it is to be renewed by withdrawing the tongue from the palate, and again replacing it, as in pronouncing the word *tut*. It will be advisable to practise the keeping up of a stream of air in this manner with the blowpipe alone, without attempting to apply it to a flame, and having become tolerably expert, the learner may then proceed to try to keep up a steady flame, without attempting to direct it on any object, and when he can do this with facility, he may proceed to the oxidation of metals, the reduction of ores, and the fusion of some of the most fusible earthy minerals. Let him commence with the flame of the candle rather shortly snuffed, the wick being bent in the direction of the intended blast. The orifice of the blowpipe is to be held just above the bent wick, and the air gently expressed through it along the horizontal part of the wick, without touching it. The lamp or candle should be placed at such a height, that both elbows of the operator may rest upon the table while he is using the blowpipe, and the further from the mouth the instrument is held the more steady will it be.

The cone of flame produced by the blast will be found to consist of two portions—an outer and yellow flame (*a*, fig. 24,) and an inner and blue flame *b*. Oxidation is best effected at an incipient red heat, the substance to be operated upon being held at the extreme point of *the yellow flame*, where the gases derived from the decomposition of the combustible matter are burning in immediate contact with the air, and are consequently highly charged with oxygen. Reduction or deoxidation, is best effected at the point *c*, just beyond the extremity of the blue cone, where imperfect combustion is going on, and where the flame, requiring a further supply of oxygen, takes it from the metallic oxide acted upon. For this operation a blowpipe with a small orifice is to be used, and the assay must be kept quite surrounded with flame. A ragged irregular flame shows that the orifice is not round and smooth, and a cavity in the flame shows that the orifice is too large. The learner ought to begin operating upon substances that are easy of reduction, such as bismuth, sulphate of copper, and the carbonate, sulphuret, and red oxide of lead. He may then proceed to the reduction of sulphuret and arseniate of copper, and when he can keep a small grain of tin in a complete state of fusion, so as to preserve its metallic brilliancy, he may consider himself an expert operator. This affords a very good criterion for ascertaining the point at which the flame becomes an oxidating, instead of a reducing flame, for at that point the globule of tin becomes coated with a white crust of oxide.

The small fragment of a mineral submitted to examination by the blowpipe is called the *assay*, and with respect to size, it must be observed, that, except in the case of the most fusible substances, the beginner ought not to attempt to operate upon a piece much larger than a grain of mustard-seed. With respect to figure, the assay ought, if possible, to be chipped off in the shape of a small scale, with a sharp edge or point, particularly in the case of minerals that are difficult of fusion, because these fine points or edges are more easily acted upon than a thicker piece, and accordingly as the point or edge becomes rounded, or retains its sharpness, it may be considered fusible or infusible; in order to ascertain this, it is sometimes necessary to examine the assay with a lens. The earthy minerals which are fused with the greatest ease are hornblende and felspar, particularly vitreous felspar.

The operation termed roasting, or calcining, consists in submitting the metallic sulphurets, arseniurets, and other substances containing volatile matter, to a low red heat; the support used in such cases must be either charcoal, the glass matrass, the open tube, or a plate of mica. Unless the roasting be properly performed, so as to expel all the sulphur or other volatile matter, no attempt to reduce the ore, either with or without fluxes, will be attended with any satisfactory result.

Fluxes are substances added to metallic ores, or other minerals, in order to promote their fusion, and their action is either chemical or mechanical. In some cases the superior affinity of the alkaline or earthy base employed as a flux, abstracts from a metallic oxide the acid combined with it which prevents its reduction. In other cases the reduction is effected by fusing oxides with inflammable bodies, which deprive them of their oxygen, by combining with it in combustion. The flux sometimes acts mechanically, when the mineral under examination would, on its reduction, be divided into minute particles not easily collected. By mixing it with a substance easily fusible, a medium is obtained through which these particles can readily subside. They are thus prevented from being dispersed, and are collected together till they run into a globule or *prill.*

The fluxes of most general utility in blowpipe operations are borate of soda, or borax, carbonate of soda, and microcosmic salt; these must be procured in a state of perfect purity, and kept pulverized in small phials. When borax is first submitted to the blowpipe flame it becomes white and opaque, and intumesces considerably, on account of the water it contains, but on this being expelled, it fuses into a colourless globule. When a mineral is treated with this flux, the characteristics to be observed are, its degree of fusibility, and whether this takes place quickly or slowly, and whether with or without effervescence, also the colour of the glass, and the alterations which take place in its transparency. The use of carbonate of soda as a flux is twofold—first, to ascertain the fusibility of the earthy minerals, and secondly, to reduce the metallic oxides.

When soda is used with an assay on charcoal, it is very apt to be absorbed by the support, but a continuance of the heat causes it again to rise to the surface of the charcoal, or the absorption may be prevented, by adding the soda in very small quantities. Some minerals are fusible with soda only when reduced to powder, others combine with small portions of it, fuse with difficulty on the addition of more, and become quite infusible if a large quantity be added. In the examination of a mineral with soda, it ought, therefore, to be tried with different proportions of this flux, noting the effects produced by the use of each successive proportion.

Microcosmic salt, or salt of phosphorus and ammonia, intumesces considerably on exposure to the flame of the blowpipe, and boils until the water and ammonia are dissipated; it then fuses quietly into a clear globule, which continues fluid much longer than that produced by the fusion of borax, for which reason the matter to be examined can be more conveniently added. It is used for the examination of metallic minerals, with which it affords highly-characteristic indications in the colour and transparency of the fused mass.

The mode of proceeding with a fragment of a mineral to be examined by the blowpipe, is, first, to heat it gently *in the glass matrass*, observing whether it give off water, change colour, decrepitate, give off volatile matter, and whether the volatile matter be sulphur, known by its peculiar smell—arsenic, known by an odour resembling garlic—selenium, known by an odour like that of decayed horse-radish—or mercury, which will condense in small metallic globules on the cold part of the glass.

The next process is to heat another fragment of the same mineral *without fluxes or charcoal*, observing whether, on being gently heated in the outer flame, it give off volatile matter, and whether sulphur, arsenic, &c.; whether it decrepitate, become magnetic, fuse, and that wholly, partially, quickly, slowly, with a thick pasty appearance, or with the appearance of a liquid; whether it intumesce, and that violently or slightly; whether it volatilize; whether it give off fumes, and whether they be copious or slight, white, or of what other colour; whether they *leave, or do not* leave a remainder, condense, or do not condense on the support. It is likewise to be observed whether the assay colour the flame of the jet; burn either with or without a flame; whether it change colour once, or more than once; whether it be absorbed by the charcoal; whether it fuse; whether the result of such fusion be a bead of metal or a powder,—a glassy globule or a white enamel; whether the glass be transparent, or filled with air-bubbles, coloured or colourless, homogeneous or heterogeneous; and whether the enamel be smooth, or having the appearance of a frit.

A portion of the mineral, well roasted to drive off volatile matter, is then to be treated *with the different fluxes*, and its behaviour with each noted, observing all the particulars enumerated after fusion in treating of its behaviour on charcoal alone. The order in which the different appearances take place must likewise be noticed, and whether they occur on the first application of heat, or after its long-continued action. For earthy minerals charcoal is not a convenient support; they may be first tried in the matrass, and then a small scale held in the forceps (fig. 21) is to be heated, first in the outer flame, then in the inner, in order to try its fusibility. If it should prove refractory upon a long application of the inner flame, and if no appearance of roundness on the point or edges of a minute scale or splinter can be discovered through a lens, it may be considered infusible, and may then be treated with soda on platinum foil, or on the platinum wire. By this process fusion will be effected, and the tinge imparted to the glass thus produced will indicate the metallic oxide which forms the colouring-matter of the mineral.

In the little work on the blowpipe of which we have before spoken, minerals are divided into four classes, according to their habits before the blowpipe, and other characters easily ascertained. These classes are —1, combustible minerals; 2, metallic minerals; 3, earthy minerals; 4, saline minerals.

1. The combustible minerals are distinguished by these characteristics:—

They are of low specific gravity, generally below 2·0, none (the diamond excepted) being above 2·5, and some float on water.

They are all (again excepting the diamond) *soft*, yielding with ease to the knife; some are liquid.

Some of them are highly combustible at or below a red heat. The rest are all more or less combustible by the action of the blowpipe; even the diamond is slowly combustible at a heat a little below that at which silver melts.

2. The characteristics of the metallic minerals are—

Specific gravity exceeding 5·0.
Lustre metallic, when scraped.

These are in the metallic state.

Or, specific gravity less than 5·0, but more than 2·5, and they are destitute of the metallic lustre, but they are—

Reducible to the metallic state by the blowpipe, or
Rendered magnetic, or
Volatilize wholly or in part, producing a vapour, or
Communicate a colour to borax.

Some of the substances of this class are combustible, but then their specific gravity greatly exceeds that of the heaviest of the combustible minerals of class 1.

3. The earthy minerals are—

Insoluble in water.
Tasteless.
Incombustible at a white heat.
Specific gravity less than 5·0.

Destitute of true metallic lustre when scraped; are not reducible to the metallic state, nor do they volatilize at a high temperature before the blowpipe.

4. The saline minerals are—

Soluble in water.
And impart a taste.

These classes are divided into orders, and some of the orders are divided into genera, and the genera again into families.

The first class is divided into two orders, characterized by their burning with a flame, or without it.

In the second class, which is divided into two orders, the characteristics of the orders are, volatilizing or not volatilizing. The first order is divided into genera, distinguished, 1, by volatilizing wholly; 2, leaving a residue reducible to the metallic state with borax; 3, or not so reducible.

The characteristics of the families into which the genera of the first order are divided are, the absence or presence of the metallic lustre.

The characteristics of the genera of the second order are, the being or not being reducible to the metallic state, either with or without borax. The families are distinguished by the absence or presence of the metallic lustre, or by the assay becoming or not becoming magnetic after roasting.

The third class, or the earthy minerals, are divided into three orders:—1. Those that are soluble wholly, or in a considerable proportion, in cold and dilute muriatic acid; 2, those fusible before the blowpipe; 3, those infusible. The first order is not divided into genera and families; the second and third orders are divided into several genera, the characteristics of which are different degrees of specific gravity, and these genera are again divided into families characterized by different degrees of hardness, and these degrees of hardness are ascertained by the minerals—1, scratching quartz with ease; 2, scratching quartz with some difficulty, and scratching felspar with ease; 3, being as hard or harder than felspar, being scratched by quartz, and scratching window-glass with ease; 4, being softer than felspar, scratching fluor, and scratching window-glass feebly; 5, being scratched by fluor-spar.

The fourth class, or the saline minerals, are divided into two orders, the characteristics of which are, the giving or not giving, when in solution, a precipitate with a carbonated alkali. These orders are not divided into genera and families, the minerals composing them being few, and easily discriminated.

The advantages of such an arrangement as this are obvious. Let *us suppose the* student to have met with an uncrystallized earthy mineral, (massive hornblende, for instance,) and that he is ignorant of its name and composition. He tries it with the blowpipe, and finds it is not combustible, and it is not soluble in water, and has no taste; it must, therefore, belong either to the metallic or to the earthy minerals. It has not the metallic lustre when scraped, nor is it reduced to the metallic state, nor rendered magnetic, nor volatilized by the blowpipe, nor does it impart a colour to borax: it cannot be a metallic mineral. His search for a description answering to his specimen may, therefore, be confined to the earthy minerals, and by the division of this numerous class into orders, genera, and families, the labour of this search is further abridged. This specimen is insoluble in acids, and it is fusible; it therefore belongs to the second order of this class; its specific gravity is 3·3. It therefore belongs to the first genus of that order, the characteristic of which is, that the minerals composing it possess a specific gravity above 3·0, and below 5·0. It scratches fluor-spar, and its action on window-glass is feeble: it therefore belongs to the fourth family. He has now only to examine, in detail, the descriptions of nine species, with none of which there is any danger of confounding common hornblende, except augite and hornblende slate. From the latter it is distinguished by the absence of the slaty structure, and from augite by its greater fusibility. Augite and hornblende are better distinguished by the forms of their crystals: they are, in fact, very closely allied, as we shall have occasion to notice hereafter, and are among the few minerals which are not easily discriminated without the aid of the crystallographer.

ON THE ROTATORY MOTION OF CAMPHOR UPON THE SURFACE OF WATER.

There is in science a number of well-known isolated facts, which seem, at first view, to contradict established principles, or, at least, to require for their explanation a train of causes not generally recognised; each fact, therefore, becomes loaded with many theories, and it is difficult to avoid mixing together principles which have really no necessary connexion with the phenomenon, so that the philosopher finds himself frequently unable to give a simple answer to the simple question, "What is the cause of this fact?"

It is possible that our causes are in most cases but removed effects; that is, we explain one effect by another a little more remote, and then the latter is termed a principle, and fairly so, since science does not pretend to teach first causes, but to lead the mind, by slow steps, gradually nearer to the only First Great Cause of all created nature.

Among the isolated facts of which I have spoken, is the rotation of camphor on the surface of water; an effect, small indeed, but curious and interesting, and which has claimed the attention of many eminent philosophers, and excited the curiosity of the more humble student. I will briefly mention the principal hypotheses which have appeared on this subject, and then offer the results of my own observations, together with the train of reasoning which has been suggested thereby.

If a few fragments or thin shavings of camphor be thrown on the surface of clean water, they will instantly begin to move, and acquire a motion both progressive and rotatory, which continues for a considerable time. During these rotations, "if the water be touched by any substance which is at all greasy, all the floating particles quickly dart back, and are, as if by a stroke of magic, instantly deprived of their motion and vivacity."—ACCUM.

The motion of the camphoric particles has been attributed by Lichtenburg to the emanation of an æthereal gas from the fragments of camphor; but he confesses that the cause is involved in considerable obscurity. Venturi supposes that a dissolvent power is excited on the camphor at the common margin of the air and water: he cut pieces of camphor into the form of small columns, an inch in length, and fixed a piece of lead to the base of each column; they were then placed upright in clean saucers, and pure water poured in to half the height of the column. A few hours after, an horizontal notch was seen in the column of camphor at the *surface* of the water; and in twenty-four hours the camphor was cut in two at the middle.

Venturi thinks that the camphor at the surface of the water dissolves, and extends over its surface; and by this means coming into contact with a large atmospheric surface, is absorbed and evaporated: the rotatory motion he refers to the mechanical effect of the re-action which the camphoric liquor, extending itself upon the water, exerts against the camphor itself: if the retro-active centre of percussion of all the jets do not coincide with the centre of gravity of the solid camphor, a combined motion of rotation and progression must follow. As the departure of the camphoric solution takes place only at the surface of the water, the rotation is necessarily effected round an axis perpendicular to the horizon.

The theory of Venturi was not considered adequate; and electricity (so commonly the high priest of scientific enigmas) was supposed, by others, to be disturbed, the moment the camphor fell upon the surface of the water. Others again thought that the evaporation of the camphor and water explained the cause; and within the last two or three years Matteucci has examined the subject, and thinks that the camphor upon water resembles potassium under similar circumstances; the liberation of hydrogen and the vapour of water around the floating vessel, producing its rapid motion.

He took rather a large piece of camphor, in order that its motion on water might be slow: under the receiver of an air-pump in a partial vacuum, the movements of the camphor, which were at first very slow, became more rapid, and ceased when the action of the pump was stopped. Matteucci says, "I have observed these phenomena of rotation on water in all volatile bodies. I took raspings of cork, and impregnated them with sulphuric æther; when placed upon water, these small light bodies turned very rapidly." Matteucci's conclusion is, that the rotation of *volatile bodies is owing* to the currents of their vapours; but this opinion, though published so recently, is by no means new; several years ago M. Biot examined the subject in connexion with an investigation of it by Prevost, and promulgated a similar opinion to that of Matteucci. M. Biot considers that camphor is moved upon the surface of water by the effect of the emission of the particles which compose it; an emission that becomes perceptible to our senses by the smell which it produces, and by the repulsion which it exercises against small bodies floating upon the surface of the water. As the effect resulting from these different impulses does not necessarily pass through the centre of gravity of the piece of camphor, this centre has a motion in most cases both progressive and rotatory.—*Annals of Philosophy*, vol. XXIV., p. 75; and *Nicholson's Journal*, vol. I., p. 51.

This curious subject has at various times attracted my attention in the progress of different chemical experiments in which camphor was concerned. I have examined it in various ways, and confess that I feel more diffidence than doubt in offering my views and the experiments which led to them, to the notice of the readers of this journal. But as the experiments of the most humble inquirer, whose object is the attainment of truth, must at all times be worthy of some notice, I feel in this a sufficient apology for stating my opinions in opposition to, confirmation of, or even objection to, those of the eminent men above noticed.

I would, then, account for the motion of the camphor upon the surface of pure water on the known principles of capillary attraction; and the experiments I now proceed to offer, support, I think, that view.

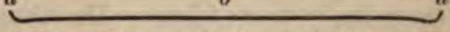
It has been shown by Sir David Brewster, that highly-expansive fluids and vapours are pent up within the cavities and pores of gems and precious stones; and the remark may probably be extended to a large number of solids of a crystalline nature: the composition of this fluid or vapour, in many cases, is probably identical with that of the substance containing it. Now in a porous, vaporizable substance, like camphor, the pores are, in all probability, filled with camphoric vapour; and upon placing a thin lamina of the substance upon water, the substitution of water for vapour in the cavities, I conceive, occurs as follows:—The minute pores act the part of capillary tubes, and attract the water into them, which water necessarily expels the vapour previously existing therein: this expulsion of vapour has an effect analogous to that of the jet from a centrifugal pump; that is, to bear the camphor round on a vertical axis. As it is a mere chance whether the forces on opposite sides of the centre of gravity equal each other, the effect, in nearly every instance, is to give the rotatory motion to which I allude: sometimes it is both progressive and rotatory; then it will suddenly change to a rotation in the opposite direction; all depending on the relative forces of the different little currents.

Matteucci states, that while the camphor is rotatory, if the vessel be covered with a glass plate, the rotation is stopped; but I find this to be the case to a certain extent only; when the vessel containing the camphor is covered, the rotations are lessened, and a general sluggishness pervades all the pieces, and the attraction of the sides of the vessel exerts itself, so that some of the pieces get to the side, and gently oscillate. I attribute this to the formation of vapour of camphor, which, accumulating between the under-surface of the glass plate and the surface of the water, prevents the further liberation of camphoric vapour, and thereby considerably lessens the capillary attraction of the water; but in no case did I get an entire suspension of rotation or movement.

I have succeeded in imparting motion to raspings, or, what is better, to thin slices, of cork steeped in sulphuric æther. I think we may apply the same reasoning to this instance as to the former. The slices of cork were steeped in æther for two or three days in a closed bottle; I then placed a few slices on the surface of water, when they rotated for several minutes, and did so, I think, while in the act of exchanging their æther and vapour of æther for water, and the effect ceased when they had no more æther to exchange for water, since it is obvious that in both cases each slice of cork was saturated with a liquid,—*i. e.*, with æther in the first instance, and with water in the second.

I agree with Matteucci in the observation, that under the receiver of an air-pump, while the air is being withdrawn, the gyrations are quicker; but I do not agree that the increased velocity is due to evaporation, but simply to the more copious escape of the camphoric vapour, and the increased capillary action under such circumstances, by which means the pores become filled with water, and the camphor cannot again be made to rotate. This increase of emissive force I consider to be due to exactly the same source as the more rapid ebullition of hot water, when deprived, either wholly or partially, of atmospheric pressure.

If the exhaustion be carried on too far, the pieces of camphor are attracted by, and cling to, the interior surface of the vessel, and remain attached thereto at the level of the water: on re-admitting the air, they instantly recede from the vessel, as if they were repelled by a force; but they do not again rotate. Now, in order to explain this, I must premise that when water is in a vessel whose sides above the liquid level are wetted, the attraction of the glass for the water is such, that a portion is elevated at the circumference of the liquid surface, so that a vertical segment of the liquid would give a line thus:—



The water is elevated at *a a*, where it is in contact with the glass, and slightly depressed at *b*, by virtue of this attraction, as also by atmospheric pressure; a slice of camphor, then, floating upon the liquid surface, is attracted by the sides of the vessel at *a a*, but this attraction is so slight that, in consequence of the ascent of the fluid at *a a*, the camphor cannot touch the glass at any one point; but if the atmospheric pressure be at all concerned in slightly depressing the surface at *b*, and assisting the elevation at *a a*, it is obvious that the removal of the whole or a part of that pressure will remove the depression at *b*, and lessen the elevation at *a a;* the attraction, then, of the sides of the glass for the floating camphor is most favourably exerted, and consequently they dart to the sides, and there remain, while the re-admission of the air restores the first state of things, and the camphor quits the sides of the vessel for the same reason that a smooth solid slides down an inclined plane.

If the production of the gyrations of the camphor are to be referred to capillary attraction in the first instance, and to the escape of camphoric vapour in the second instance, by whose means currents acting like paddles constitute the moving power, it is obvious that heat would assist the liberation of the vapour, and produce more rapid rotations, whose career would terminate much sooner than at the ordinary temperature of the air. All this I find to be the case.

Pure water was heated to 148°, when the rotations of the camphor were increased in velocity, and ceased entirely in sixty-nine minutes.

Two glasses were set aside, one containing water at 58°, and the other at 210°; several slices of camphor were placed in both at the same time; the camphor in the first glass rotated for above five hours, until all but a very minute portion had evaporated, while the rotations of the camphor in the hot water lasted only nineteen minutes; about half the camphor had passed off, and the remaining pieces, instead of being dull, white, and opaque, were vitreous and transparent, and evidently soaked with water. The gyrations, too, which at first were very rapid, gradually declined in velocity, until they were quite sluggish.

The stilling influence of oil upon waves has become proverbial; the extraordinary manner in which a small quantity of oil instantly spreads over a very large surface of troubled water, and the stealthy manner in which even a rough wind glides over it, must have excited the admiration of all who have witnessed it.

Now it is by the same principle that we must account for the "magical" action of a drop of oil in stopping the rotations of the camphor, which action is best shown in the following manner:—throw some camphor, both in slices and in small particles, upon the surface of water, and while they are rotating, dip a glass rod into oil of turpentine, and allow a single drop thereof to trickle down the inner side of the glass to the surface of the water; the camphor will instantly dart to the opposite point of the liquid surface, and cease to rotate. This is due to the rapidity with which the oil spreads over the surface of the water, and I apprehend that each particle of camphor becomes surrounded at the water's edge with a minute film of oil, which prevents further contact with the water, and the consequent progress of capillary attraction, and the formation of the currents I have spoken of. If a greasy solid, such as hard tallow or lard, be employed, the motions of the camphor are more slowly stopped than by oil or fluid grease.

A few drops of sulphuric or muriatic acid gradually stops the camphor's motion, but when camphor is dropped into nitric acid, diluted with its own bulk of water, it rotates rapidly for a few seconds, and then stops.

The camphor rotates in a strong solution of liquor ammoniæ, as well as in water, but it does not rotate in various solutions of salts.

I find also, that sublimated benzoic acid rotates upon water, though in a manner far less decided than camphor, and for a much shorter time.

Having now endeavoured to account for the motions of the camphor, I will speak of the before-mentioned currents.

By attentively examining with a lens, and in a good natural light, a piece of camphor while rotating, the currents can be well distinguished jetting out, chiefly from the corners, of the camphor, and bearing it round. The motion is by no means equable; sometimes it is slow, when the currents are small and weak, but often very rapid, when they are strong: sometimes a large current will suddenly burst forth, and produce a rapid eccentric motion; it is the irregularity in the force of these currents which causes the fluctuating and flitting changes in the motions for an instant; a balance of force will engender momentary rest, which is, however, immediately disturbed by some new current darting out, and the direction in which it will rotate is always dependent on the aggregate strength of the current at any given spot.

An egg placed in dilute muriatic acid, at first sinks in the solution, but in a few seconds, the whole of the egg-shell being covered with bubbles of carbonic acid gas, will rise to the surface; a portion of the egg will be lifted above the surface, and the whole egg will slowly rotate upon its prolate axis. This rotation is formed by the bubbles of gas forming at the under part of the egg, and over all the submersed portions, which render them lighter than the portions above the liquid level, and this portion descends as the other ascends. The instances in chemistry of solids moving rapidly through liquids, are numerous. Almost any soluble salt, if thrown into a nearly saturated and boiling solution of the same salt, will rotate in dissolving.

The currents as given out by the camphor may also be seen by means of the microscope; a drop or two of pure water may be placed upon a slip of glass, with a particle of camphor floating upon it. By these means the currents will be detected, and it will be seen that they cause the rotations.

Or a flat watch-glass, called a *lunar*, may be employed, raised a few inches, and supported on a ring formed out of a piece of wire, and kept steady by thrusting one end into an upright piece of wood, like a retort-stand. The watch-glass is to contain the water and camphor, and a sheet of white paper is to be so arranged below it as to receive the shadow of the glass, camphor, &c., to be cast by a steady light placed above, and a little on one side of the watch-glass. On observing the shadow, which may be considered a magnified representation of the object itself, the rotations and currents can be distinguished.

It may perhaps be thought, that the motion of a bit of camphor is too insignificant to dwell upon; but experimental philosophers know the value of small facts, when viewed as the stepping-stones to enlarged and general principles; and the fact that Biot, Prevost, Venturi, and Matteucci, have not thought it beneath them to examine these curious phenomena, will, I hope, furnish a sufficient apology for a more humble labourer in the same field.

A POPULAR COURSE OF ASTRONOMY.

No. X.

THE MOON'S DISC.—ECLIPSES.

SEEN by the naked eye, certain portions of the moon's disc appear darker than the rest, and viewed under the telescope, the whole of its surface seems covered with irregular markings. These markings may be divided generally into two distinct classes,—those which are *permanent*, presenting precisely the same forms and intensities at all ages of the moon, and those which vary continually in form and intensity, with the varying elongation. The latter have before been stated to be manifestly the shadows of mountains and of cavities, varying perpetually in length, with the variation of the angle at which the sun's rays fall upon those irregularities of the moon's surface of which they are the shadows. For let it be observed, that by reason of the continual revolution of the moon upon an axis within itself, in the period of a synodic revolution about the earth, the light of the sun is made to fall upon any object on its surface, at every possible angle within certain limits, in the course of the half of that period, or about fifteen days,—precisely as, by the daily revolution of the earth upon its axis, the sun-light is made to fall upon an object on the earth's surface at an infinite variety of different angles within the period of a day, and thus to give shadows of an infinite variety of different lengths.

From the fact of the other class of marks to which we have referred not presenting this variety of dimensions with the moon's age, we conclude therefore certainly that they are not shadows; but that they result from some peculiarity in the nature of the surface of the mass of which the moon is there composed, affecting the reflection of the sun's rays, and absorbing more of them there than elsewhere.

Some have supposed them to indicate the presence of a fluid. This conclusion, however, is not a necessary one; and it seems to be contradicted by the fact that the moon has no atmosphere,—a fact which is supposed to be established. It seems pretty certain, that a liquid of the nature of those which exist at the earth's surface would, were the atmospheric pressure removed, very soon convert itself into vapour, under the influence of the heat of the sun, and, *à fortiori*, this result might be expected on the moon's surface, where the elevation of temperature resulting from each fifteen days of continued sunlight must be enormous.

The principal of these marks have been very accurately observed and laid down upon a map of the moon by Schroeter; and he has given to them the names of Aristarchus, Manilius, Eudoxus, Tycho, Copernicus, Kepler, &c. One of the most remarkable is that called Aristarchus, on the eastern limb of the moon. Luminous points, having the appearance of sparks of fire, have been seen in this spot by Cassini, Herschel, Kater, &c., even at the period of new moon. Of the action of fire on the surface of the moon there are said to be numerous evidences: the cavities have all the appearance of craters; all round their edges appears to be thrown up in immense ridges the matter which must have been removed to form them. These ridges Herschel asserts to be visibly stratified; their form is hemispherical, or cup-shaped; and from the bottom of one cavity thus formed a cone-shaped hill will not unfrequently be seen to uplift itself, bearing at its apex another cavity. These are all indications of volcanic action, precisely analogous to indications presented by certain portions of our globe. It requires, indeed, no great stretch of imagination to conceive a period when our earth presented a surface similar to that of the moon, from the bare unfruitfulness of which it has subsequently progressed to its present state of beauty and productiveness.

It has been stated as a fact which is commonly asserted and believed, that the moon has no atmosphere: it has moreover been stated to be a received opinion that volcanic action has heretofore taken place to a great extent on the surface of the moon, and indeed, that fire may now occasionally be seen upon it. Now, these opinions appear at first sight to be incompatible; we have no notion of combustion without air,—air being, in fact, that compound substance, from the decomposition of which combustion with us at the earth's surface results.

Although there is manifestly a difficulty in this, yet it is not in reality so great as at first sight it may appear to be. Oxygen gas is that constituent of the air which is thus required for combustion; and it is ascertained that there are certain bodies, which, in the course of their decomposition, supply sufficient of this principle for their own combustion.

That the moon has no atmosphere is said to be proved by the fact, that the time of the occultation of a fixed star would not be that which we observe it to be, if such an atmosphere existed. An atmosphere like ours must, as will be afterwards explained, have the power of refracting, or turning at an angle, the directions of the rays of light falling from such a star obliquely upon it: when, therefore, the direct rays of the star are intercepted by the intervention of the moon's body, certain others would, by means of this refraction, be so turned as to come into the eye, and the star would be visible even when it was behind the moon: thus we should see it for some time after the moon has in reality covered it, and for some time before it leaves it; and the length of the obscuration would thus be less than the time actually necessary for the moon's disc to pass over the star; which time may be calculated from the known period required by the moon to pass over a degree of the heavens, and the known angular width of the disc at the point where the occultation takes place. There are, indeed, certain densities of the lunar atmosphere which may be conceived such that a star should be visible throughout the whole occultation, or, rather, that there should be no occultation at all.

All this reasoning, and it is the only reasoning on which the conclusion that there is no lunar atmosphere is founded, proceeds on the supposition that the existence of such an atmosphere necessarily implies the existence of a power of refracting light, analogous to that of our own atmosphere.

If there be no atmosphere on the moon, it is difficult to conceive the existence of living beings upon it, either vegetable or animal. Do such living beings, however, exist there, and did we think it worth while to speculate about them, reasoning by analogy, we might arrive at the conclusion that their strength and stature must be less than ours. All those objects for which a certain share of strength and stature are given to us, could by them be effected with about one-sixth of these. The force with which bodies are attracted to the surface of the moon is about one-sixth of that by which they are attracted to the centre of the earth: so that any object which a lunarian might wish to lift he would lift with one-sixth the muscular exertion required to lift it here, and to accomplish the same purpose he would require only one-sixth the muscular strength. The surface of the moon too is only $\frac{3}{40}$ths, or about $\frac{1}{13}$th that of the earth, and its circumference $\frac{3}{11}$ ths, or about one-fourth, so that he might obtain the same proportion of locomotion on its surface, and appropriate to the uses of his own existence about the same proportion of it, with about one-sixth or one-eighth the same actual change of place. His limbs need not then be of the same dimensions; and if the vegetable and other animal existences around him be adjusted to the same scale, his stature need not be so high, or his bulk so great:—but enough of these idle speculations.

We shall pass now to the discussion of certain other appearances of the moon's disc. In the first place, then, it is observed not, at all periods of its age, to be exactly of the same size. If measured accurately with a micrometer, its diameter will be found in the course of a single lunation to vary from 29′ 21″·91 to 33′ 31″·07. This is a much greater variation in the angle which it subtends to an observer on the earth, than the similar variation which takes place in the apparent diameter of the sun in the course of a year*. Now, if any number of apparent diameters intermediate to these be observed, and also the corresponding longitudes, precisely the same relation will be found to exist between them as was found to exist in the case of the sun, from which relation it was shown, that the distances of the earth from the sun were those of the circumference of an ellipse from its focus. Moreover, the law will be found to obtain, that the angle described in a given small time, an hour, for instance, being divided by the square of the apparent diameter, or, which is the same thing, being multiplied by the square of the radius vector, the result is always the same. From these facts, then, we deduce the conclusion, that the moon moves round the earth in an orbit which is not a circle, but an ellipse, or very nearly so; and that its motion in that ellipse is governed by this remarkable law, that the area described or swept over in a given time by the radius vector is always the same; the orbit of the moon round the earth precisely resembling in these respects the orbit of the earth round the sun, but having a greater eccentricity.

But not only is there this variation in the apparent diameter of the moon at different ages during the period of the same revolution, but there is an analogous variation in her apparent diameter at the same ages of different revolutions. Thus, for instance, if the moon's apparent diameter be observed when she is full for a succession of different lunations, it will be found, that in a certain period the diameter will go through every possible variety of change between the extreme limits of 29′ 21″·91 and 33′ 31″·07, being in no two successive lunations the same, and returning to the same value only after the expiration of this period.

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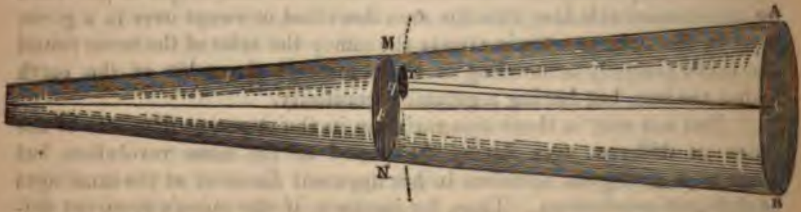
* The diameter of the sun varies from 31′32″ to 32′35″; so that the moon is sometimes apparently less, and sometimes greater, than the sun.

ECLIPSES OF THE MOON.

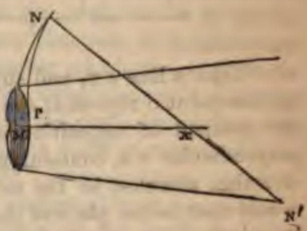
THE earth being an opaque body, a certain portion of the light of the sun is intercepted by it, and a dark shadow projected behind it in space. Being of less dimensions than the sun, this shadow is cone-shaped, having its apex in that line produced which joins the centres of the earth and sun, and for its base that section of the earth through its centre which is perpendicular to this line. Moreover, this cone, if produced, would have a section of the sun for its base. The cone, therefore, is such, that sections of the earth and sun would be sections of it at a distance equal to that of the sun from the earth. Hence, knowing these sections and this distance, we can tell what are the precise dimensions of the cone, and the distance of its apex from the centre of the earth is thus found to vary (as the earth's distance from the sun varies) from 212 to 220 radii of the earth, or from 838,500 to 870,003 miles. Moreover, the moon's distance from the earth is not more than 237,000 miles. Hence, therefore, if the moon's motion round the earth were in the plane of the ecliptic, or in that plane which is swept out by the line joining the centres of the earth and sun, then at each revolution its centre would cross that line; it would pass through the very axis of the cone and the centre of the shadow, and the section of the shadow being there somewhere about seven times and one-ninth the section of the moon, it follows that the moon would be for some time wholly immersed in the shadow, and thus receiving no light from the sun, would be invisible to us, or eclipsed, as it is termed, once in every successive lunation. Moreover, supposing the moon's orbit not to be in the plane of the ecliptic, as it really is not,—if, nevertheless, its inclination were such as to cause the moon to pass within a distance of one of its radii from the surface of the shadow, then, as before, would a part, if not the whole of the moon, be carried at each revolution into the shadow, and a certain part, or the whole, receiving no light from the sun, would be invisible. We may easily find what must be the limit of the inclination of the moon's orbit, that an eclipse may thus take place at every successive lunation.



The accompanying figure represents this shadow of the earth in its correct proportions. A B is that section of the earth which is its base, M N the section of the shadow at the distance of the moon's orbit, and *q r* a *section of* the moon.

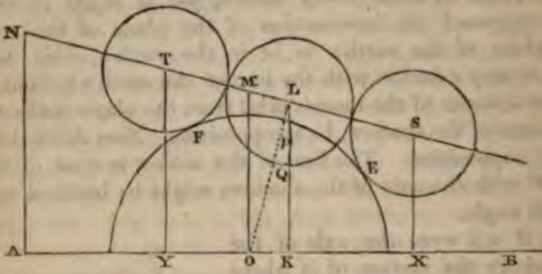
If we add together the angles *p* C *q* and *q* C *r*, we manifestly get *p* C *r*, which is the inclination of the orbit in which, if the moon revolved, her disc would at every revolution just touch the surface of the cone. This inclination is therefore the limit of all those at which an eclipse could take place at each revolution. Now the angle *p* C *q* is that made at the earth by the radius of the section of the shadow, where the moon enters it, and is easily calculated to have for its least value 37′ 42″. Also, the angle *q* C *r* is the apparent semi-diameter of the moon, and its least value is 14′ 41″. So that the sum of these, or 52′ 23″, is that inclination, which, if the inclination of the moon's orbit did not exceed, there would be an eclipse every month.

Although, if the inclination exceed this limit, an eclipse does not thus *necessarily* occur every month, yet it *might* so occur. We have here supposed the intersection of the plane of the moon's orbit with the plane of the earth's to be in the earth's orbit; so that the line of nodes may coincide with the line of the earth's motion, and thus the greatest distance of the moon's orbit from the plane of the earth's, and from the centre of the shadow, be in opposition. Now this is by no means a necessary supposition. The line of the nodes, instead of making an angle of 90° with the axis of the shadow, might be inclined to it only at a very small angle.



Thus, if M X were the axis of the shadow, and M P the section of it where the moon enters it, and N N′ the line of the nodes, then, whatever was the inclination of the plane of the moon's orbit, N P, the position of N N′ with respect to M X might be such, as to cause the orbit N P to pass through the shadow. And in this case, provided that N N′ and N X retained always this relative position, that is, supposing the line of the moon's nodes revolved always with the same angular velocity as the axis of the shadow does, or as the earth itself does, then would the same lunar eclipse occur at the same period of each successive lunation. But this is not the case: the angular revolution of the nodes is not the same as that of the earth in its orbit. The revolution of the earth is completed in a mean solar year; whereas the revolution of the nodes takes 18·6 such solar years to complete it. Thus, then, it is manifest, that the line of nodes does not retain the same position in reference to the axis of the shadow at each successive opposition, but that it takes every possible direction in regard to it, and therefore that there cannot possibly be a lunar eclipse every month. Nevertheless that in this variety of positions of the line of nodes there must, of necessity, be some in which it is so near to the axis M X of the shadow, that traversing its orbit N P, the moon shall, of necessity, partly, if not wholly, enter the shadow. The least angular distance of the two at which this can take place is 12° 36′, and is called the lunar ecliptic limit. If, at the time of any opposition, the line of nodes be inclined to the axis of the shadow, at an angle less than this, there must be an eclipse. The axis of the shadow being always in the line joining the earth and sun, it is evident that it revolves through the ecliptic with the sun, or with a mean motion of 59′ 8″ daily. Moreover, it is shown in our last number, (page 131,) that the regression of the line of nodes on the ecliptic is 3′ 10″ daily. Thus, knowing the rate at which these lines revolve, if we knew their position in respect to one another at any given time, we could find it at any other time. Such positions are known from observation, and the corresponding times serve as epochs whence the position of the line of nodes in respect to the shadow may at all other times be found.

Let us suppose that the position of the moon's node at the time of an opposition is thus known, and that this is, in reference to that opposition, within the lunar ecliptic limit of 12° 36′.



Take a line A B, and suppose it to represent a portion of the ecliptic, let the point O represent the place of the centre of the earth's shadow at the moment of opposition, and from any scale of equal parts take the perpendicular O M, containing as many of those parts as there are degrees, or rather minutes, in the moon's latitude* at the moment of opposition. M will then be the place of the moon's centre in opposition. And take A O to contain as many of the same parts as there are minutes in the horary motion of the moon's centre, diminished by the horary motion of the centre of the shadow (about 30′). From A draw the perpendicular A N, equal on the same scale to the moon's latitude in one hour after opposition. If M N be then joined and produced, it will be what is called the moon's relative orbit. Being an orbit which, if it described, it would come precisely into the same positions with regard to the centre of the shadow O (which is supposed at rest) as it actually does come into with respect to that point in motion. Draw the perpendicular O L; then will this be the shortest distance of the point O from the moon's orbit, or the shortest distance within which the centre of the moon comes to the centre of the shadow; and if we ascertain how many equal parts of our scale are contained in this line, we shall know how many minutes there are in the least apparent distances of the centres of the moon and shadow. From the centre L, with radius equal to as many equal parts as there are minutes in the moon's semi-diameter, describe a circle: it will represent the space which the moon's disc covers in the heavens. It remains now only to find the number of minutes in the apparent semi-diameter of the section of the shadow; and when this is known, to describe from O a circle E F, having a radius of as many equal parts as there are minutes in this: the space of the heavens covered by the shadow will be represented by this circle, and the amount of the eclipse ascertained by its interference with that which represents the moon's disc.

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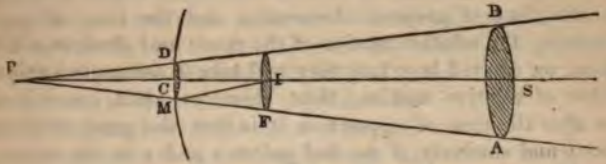
* These data may all be found in the *Nautical Almanac*.

Now the angular semi-diameter of the section of shadow is equal to the moon's horizontal parallax, diminished by the sun's apparent semi-diameter. This is easily proved.



Let A P B represent the shadow, I the centre of the earth, and M D the section of the shadow; then will C I M be the angle under which the semi-diameter of the shadow is seen from the earth; or it will be that apparent semi-diameter of the shadow which we want. Now, since I M F is the exterior angle of the triangle P M I, therefore it is equal to the two interior angles M P I and M I P; and therefore P I M is equal to F M I diminished by M P I or A P S. Now, I M F is the angle under which the earth's radius would be seen from the moon; therefore it is the moon's horizontal parallax; and A P S is the angle under which the sun would be seen from the apex P of the shadow. Now, since the sun's diameter is exceedingly great as compared with that of the earth, it follows that the apex of the shadow is exceedingly near to the earth as compared with its distance from the sun; that is, because A S is very great as compared with I F, it follows that I P is very small as compared with I S: in point of fact, it will be found that whilst the distance of the earth from the sun is about 214 radii of the sun, that of the apex of the shadow from the earth is only 2 radii of the sun; thus, then, I P is only $\frac{1}{107}$th part of I S; and this being the case, it follows that the angle under which the sun would be seen from P is not sensibly different from that under which it is seen from I; the angle A P S is therefore very nearly equal to the sun's apparent semi-diameter, and it follows that the radius of the section of the shadow is very nearly equal to the moon's horizontal parallax, diminished by the sun's apparent semi-diameter. Both these quantities are variable, but they are stated for every day of the year in the tables of the *Nautical Almanac*, and other ephemerides. Take them, then, corresponding to the time of the opposition, subtract them, take in the compasses as many equal parts from the scale as there are minutes in the difference, and describe a circle having the point O for its centre (fig. page 216): if this circle intersect that which was before described, having L for its centre, there will be an eclipse; if not, there will be none. Moreover, the quantity of the interference of the two circles will mark the degree of obscuration when it is greatest: thus, in the figure, the two circles over-lapping one another by a distance equal to P Q, if this distance be measured on the scale of equal parts, as many of those parts as are contained in it, so many minutes of the moon's disc will be eclipsed.

But the usual method of designating the amount of the eclipse is to divide the whole diameter of the disc into twelve equal parts, called digits, and ascertain how many of these are in the width, P Q, of the obscured portion.

To find the exact time of greatest obscuration, we have only to draw from L the perpendicular L K on A B, and ascertain how many parts are in O K; this will give the number of minutes of longitude through which the moon's centre moves, in reference to the centre of the shadow, between the time of greatest obscuration and the time of opposition. Now, knowing the relative motion of the moon and shadow in longitude for an hour, we can tell how long they will take to accomplish this number of minutes of relative motion; thus, then, we shall know how much before or after the time of opposition it is that the greatest obscuration took place; and similarly, if we find points T and S in the relative orbit N S, such that circles described from these points representing, as before, the moon's disc, may just *touch* the circle which represents the shadow, and having found those points T and S, if from them we draw perpendiculars, T Y′, and S X, upon A B, then O X and O Y will represent the relative motions in longitude of the moon and shadow between the time of opposition and the times of the commencement and termination of the eclipse respectively; and knowing the hourly amount of this relative motion, we can find, as before, the time from opposition of the commencement and end of the eclipse. Thus all the circumstances of the eclipse may be determined with considerable accuracy by a very simple construction, and the most elementary processes of arithmetic.

Let us now proceed to the subject of eclipses of the sun.

ECLIPSES OF THE SUN.

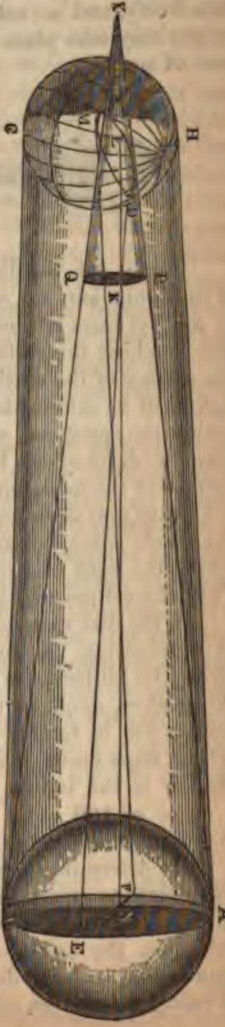
NOT only does the earth, being an opaque body, throw into space a dark shadow, which, by reason of its diameter being greatly less than that of the sun, is of the form of a cone, terminating in a point whose distance is from 838,500 to 870,003 miles from the earth's centre, but which is, although so enormous a distance, nevertheless only $\frac{1}{107}$th of that of the sun; but the moon, also, for like reasons, darts her conical shadow outwards into space,—a shadow whose termination is at a less distance from her than the earth's from the earth's centre, in the proportion in which she is less when compared with the sun, than the earth is, compared with the sun.

Since the termination of the moon's shadow is formed by the intersection of lines drawn touching the opposite extremities of diameters of the sun and moon, it follows that to an observer at a distance from the sun equal to that of the termination of the shadow, the sun and moon would appear precisely under the same angle; and conversely, if the sun and moon appear under the same angle to an observer, then he knows that he is at the same distance from the sun that the apex of the shadow is. Now, the moon appears to us sometimes under precisely the same angle as the sun, sometimes under a greater angle, and sometimes under a less: we know, then, that we are sometimes at the same distance from the sun that the apex of the shadow is, and in the *very place* where it would be, if the moon came between us and the sun; and we know, also, that we are sometimes at a less, and sometimes at a greater distance. Now, we know that by reason of the small inclination of the moon's orbit, and the motion of its nodes, it must sometimes intervene in a line between us and the sun; we know, then, that we may be, and shall be sometimes, in the very apex of the shadow; or plunged some depth into the conical extremity of it; or that, in other cases, the apex of the shadow must lie in a line between us and the sun, but nearer the sun. In the conical shadow there is absolutely no light, (except, perhaps, some little reflected from the earth, called earthlight;) when, therefore, we are in the apex of the shadow, or immersed any depth in it, we can receive no light, and the sun must be invisible, or there must be a total eclipse. Thus it is proved that the shadow of the moon is of such a length as sometimes to be long enough to reach the earth, and that its motion is such as to sweep this shadow, which is thus long enough, sometimes across it. The greatest area which the shadow of the moon can cover upon the earth is a circle of about 180 miles in diameter; the actual spot thus covered is, of course, perpetually varying with the relative motion of the earth upon its axis, and the synodical motion of the moon.

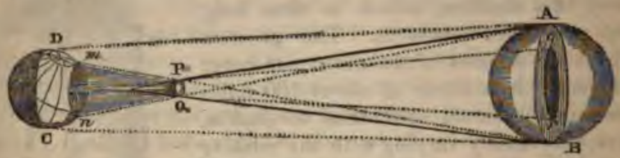
Such are the conditions of a total solar eclipse. It is manifest that such an eclipse can be visible only at those places over which the tail of the shadow is made by the combined rotatory motion of the earth and the synodic motion of the moon to sweep. This is not, however, by any means the most general way of looking at the question: an infinity of eclipses of the sun may occur besides the total eclipse; and these may be brought about without the apex of the shadow anywhere coming in contact with the earth's surface.

Let us consider the more general conditions of a solar eclipse.

Let A B C H represent the cone of light which falls from the sun upon the earth. Now it is clear that the moment the moon, or any portion of the moon, enters this cone, a portion of the light which falls upon the earth must be intercepted somewhere or another. A portion, or the whole, of the rays which fell before from the sun do not now fall there. Let P Q represent a section of the moon partially immersed in the cone of sunlight; and let M D represent the intersection of its shadow with the surface of the earth; then it is clear that at every place within the space M D a portion



of the sunlight will be intercepted. At L this portion may be ascertained by joining L K, and producing this line to N, K being any point on the circumference of the moon's disc. No light coming from the portion of the sun N E can reach an observer at L; nevertheless the whole light between N and F will come freely: thus the sun will be eclipsed to the observer at L to the extent E N; and if the point K be supposed to traverse the circumference of the moon's disc, the point N will trace out a line A N B on the sun's disc, which will be the boundary of its bright and its obscured part, as seen from L. To observers at points between this and D, it will be similarly eclipsed, but to an extent less than this, until at D the very edge of the solar disc only is interfered with, and scarcely any eclipse is visible; and on all points without M D the sunlight falls freely, and no eclipse is seen: thus, then, it appears that a partial eclipse may take place when the moon is not wholly immersed in the cone of sunlight.



Again, let the disc P Q of the moon be wholly included in the cone of sunlight; join A Q, and produce it to *n*, and join B P, and produce it to *m*, and let lines be similarly drawn from all the other points in the sun's disc touching the surface of the moon, and enclosing the portion *m n* of the surface of the earth. Then, without this space *m n*, none of the sunlight will be intercepted, and no eclipse will be visible. At any point *h* within the space *m n*, a portion of the sunlight will be intercepted, which may be determined by drawing lines *h* P and *h* Q, &c., touching the surface of the moon, and producing them to the sun's disc in *q r*, &c. It is evident that these points, *q r*, &c. will lie within the sun's disc, and that the whole space included by them will be obscured to the observer at *h*, whilst the light will come freely to him from the space without them. Thus the eclipse will be to him an annular eclipse, the circular portion, *q r*, of the disc only being obscured. If, however, the shadow, instead of terminating, as shown in the figure, at a distance from the earth, sweep its surface, the space *q r* will, if *h* be within the area covered by the shadow, swell so as to include the whole solar disc A B, and the eclipse will be total. The space *m n*, within which an annular or total eclipse must occur, is called the penumbra, as that covered by the actual shadow is the umbra. It has been shown that the umbra may cover a space of 180 miles diameter on the earth's surface; the penumbra may cover 4,900 miles.

In determining the circumstances of a solar eclipse, the first step is manifestly to ascertain the precise moment when the moon first enters the cone of sunlight, so as to intercept the rays which would otherwise fall upon the earth,—to ascertain, further, the moment of its greatest immersion, and the depth of that immersion, and lastly, the time when *it emerges* from the cone of sunlight. These points being determined, it will yet remain to fix upon all those places of the earth's surface where the rays intercepted during the period of this immersion would otherwise have fallen; that is, to determine the various places where the eclipse will be visible. The determination of the time when the moon first enters the cone of sunlight bears a close resemblance to that of the time of its entering the earth's shadow, which we have described in treating of eclipses of the moon: in fact, the shadow is (geometrically) but a continuation of the cone of sunlight, so that, in fact, the circumstances of the entrance of the moon into the earth's shadow, and its entrance into the cone of sunlight falling from the sun to the earth, are but the circumstances of its entrance into different portions of the same conical surface.

It is manifest, for the same reasons as those given in the case of the lunar eclipse, that this immersion cannot take place except when the longitude of one of the moon's nodes is nearly the same with that of the sun. At points of the cone of sunlight where the moon can enter it, the circular space in the heavens occupied by a section of the cone of sunlight can never have a radius greater than 1° 35′ 4″, or less than 1° 24′ 0″; and since this circle has, of necessity, its centre in the ecliptic, it is clear that the moon cannot enter at all the cone of sunlight when its latitude is greater than 1° 35′ 4″, and that it must, of necessity, enter it, if, at the period of its conjunction, its latitude is less than 1° 24′ 0″. Now, it has the latitude 1° 35′ 4″ when distant 19° from its node, and the latitude 1° 24′ when distant 13° 42′ from its node: thus, then, if at the time of its conjunction with the sun it be distant more than 19° from its node, there can be no eclipse; if it be distant less than 13° 42′, there must, of necessity, be an eclipse; and if its distance from its node be anywhere between 19° and 13° 42′ an eclipse may possibly, but will not necessarily, occur: these are called the solar ecliptic limits.

Let us now suppose it to be determined that the distance of the moon from its node at conjunction is less than 13° 42′, so that it *must* be immersed in the cone of sunlight; and let it be required to determine when the immersion does take place, and under what circumstances. In the first place, we must know the radius of the section of the cone at the distance where the moon will enter it. Now it may be shown by reasoning similar to that applied to determine the section of the shadow in the lunar eclipse, that the radius of the cone of sunlight is at this point equal to the apparent semi-diameter of sun + moon's horizontal parallax − sun's horizontal parallax. Now, the sun's apparent semi-diameter, and the sun's and moon's horizontal parallaxes, are quantities which are all given* by the tables, and may be there found for the time of conjunction: thus, then, the angular radius of the section of the cone of sunlight where the moon enters it may be determined.

Take, then, from a scale of equal parts, a number equal to the number of minutes in this angle†, and describe a circle E F (see fig. page 216,) having this length for its radius. Take A B passing through the centre O of this circle, to represent the ecliptic; let O A represent, on the same scale, the number of minutes in the relative motion of the sun and moon in longitude in an hour; and A N, perpendicular to A B, the moon's latitude one hour after conjunction; also let O M represent the moon's latitude at the moment of conjunction; and join M N, and produce it; then will the motion of the centre of the moon, in respect to the centre of the section of cone of sunlight, be the same as though the centre of the cone of sunlight remained at rest at O, and the centre of the moon moved in the orbit M N, M N being what is called the *relative* orbit. If from O there be drawn the perpendicular O L upon M N, intersecting the circle in P, and P L be measured on the scale so as to find how many of the equal parts used are contained in that line, then if the number of these exceed the number of minutes contained in the moon's semi-diameter, the moon will not anywhere enter the cone of sunlight, and no eclipse will anywhere be visible: if it be less than that number, there will be an immersion; the greatest quantity of that immersion being measured by the difference, P Q, of these numbers. Moreover, if points S and T be found in the relative orbit, from which circles being described, having for their radii each the number of parts in the moon's semi-diameter, will just *touch* the circle E F in the points E and F, then will these be the points where the moon first enters the cone of sunlight and where she leaves it. And if from S, L, and T, perpendiculars S X, L K, T Y, be drawn upon A B, then will the distances O X, O K, O Y, be those which the moon will have to describe, in its relative motion in longitude, between the period of its first immersion in the sunlight at E and its conjunction, between its greatest immersion at P and its conjunction, and between its conjunction and its emersion at F. Now, the space through which the moon moves in an hour, with its relative motion, being known, it is evident that we can find the time which it will require to describe with that motion these spaces O X, O K, O Y: thus, then, we shall find the times from conjunction of the first immersion of the moon in the cone of sunlight, of its greatest immersion, and of its emersion.

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* The sun's horizontal parallax may be considered constant and equal to 9″; the greatest and least values of the moon's horizontal parallax are 62′ and 53′; and the greatest and least apparent semi-diameter of the sun are 15′ 45″ and 16′ 18″.

† Its mean value may be taken at 73′¼.

Throughout the period whilst the moon is thus partly or wholly within the cone of the sunlight, it is obstructing some of the rays which would otherwise fall from the sun to certain parts of the earth's surface, and producing there a partial or perhaps total eclipse. It remains now to examine what are those places of the earth where any eclipse is visible, and under what circumstances. For this purpose we must discuss the question of parallax.

REFLECTIONS ON COMETS;

THEIR PROBABLE NATURE, AND THE FUTURE SERVICE THEY MAY RENDER TO ASTRONOMICAL SCIENCE.

[THE Royal Astronomical Society of London having awarded their medal of this year to Hr. Rosenberger, of Halle, for his investigations relating to Halley's Comet, the President of the Society, Mr. Airy, stated the grounds of this decision at the recent Anniversary of the Society, in the following terms.]

"I HAVE to announce to you, Gentlemen, that your Council have awarded the Society's Medal to our foreign associate, Professor Rosenberger, of Halle, for his elaborate calculations relating to the return of Halley's Comet. Permit me to point out to you the general train of considerations which have induced your Council to fix upon this matter as one of the greatest importance to astronomy, and to select this astronomer as fully entitled, by his labours upon it, to the highest mark of our respect.

"The science of astronomy, Gentlemen, is pre-eminently one of calculation and prediction—calculation of the past, and prediction of the future. The object of the first is, to extract laws and numerical elements from the phenomena that have occurred: the object of the second is, to apply these laws on the assumption of their generality, and these elements on the assumption of their invariability, to the phenomena that will occur; in order to ascertain, by comparing predicted with observed results, any error that may have been committed in these fundamental assumptions. And the history of astronomy, in all the various forms which the science has taken, constantly presents to us, either the struggle of reducing laws and elements to agreement with new phenomena, or the anxious search for some hitherto neglected causes of discordance, or, finally, the triumph of finding that assumptions were well founded, and that the agreement of prediction with observation is sufficiently exact. The last of these it has been our good fortune to witness. We have seen a comet whose last appearance it is probable that no living man can distinctly recollect,—whose period exceeds the limits of ordinary life,—whose path extends into spaces far beyond any which in other parts of physical astronomy we have need to consider,—we have seen it return within a day of its computed time, and have traced it through the heavens, describing nearly the path which had been laid down for it on our charts. I confess that the sight of this strange body, and the contemplation of the uniformity of the law which has guided its motions, and of the acquaintance with that law, and the power of tracing its effects, which man has acquired, have been to me a source of intense pleasure. And I doubt not that the same gratification has been experienced by every astronomer who has been accustomed to regard his sublime science, on the one hand as the most severe exercise of the intellect, and on the other hand as the study which leads most certainly to a knowledge of the general laws of the universe.

"There are, however, other points of view in which the subordination of this body to the general law of gravitation is extremely interesting. It is not merely that its period is long and its orbit extensive, but that it is a body of different kind, and moving in an orbit of very different proportions, from any of those which mankind had been accustomed to regard as belonging to our system. The most striking consideration is that derived from contemplation of the nebular hypothesis: a theory which, if not certain, is plausible, and accounts, in a most remarkable degree, for the phenomena that seemed to require some single cause to explain their general similarity. Is it true that this system of sun, planets, and satellites, was once a nebula, whose slow rotation and gradual condensation at length formed a number of bodies, bearing in their form and motions no traces of their original state? And is it true that comets are detached portions of nebula, which the want of mass has saved from the extreme degree of condensation that the planets have experienced; which, by the attraction of our sun, have at first been made to describe parabolas; and which, in some instances, perhaps from the effects of resistance when our system abounded with uncondensed nebular matter, have been made to return in orbits of limited extent? If these things be true, then I say that the subordination of these bodies to the law of gravitation is a most striking fact. They are a link, at the same time connecting the past with the present state of our system, and its present state with the state of those curious bodies which we find dispersed in all parts of the heavens. And their obedience to the law of gravitation affords a very strong presumption that this law has been unaltered since our system was one nebular chaos; and that it now holds in the nebulæ which preserve their state yet unaltered.

"Be this as it may, the consideration of the form of this comet's orbit suggests a different train of ideas. Comets were once regarded as monsters, as prodigies, as having no relation whatever to the order of things either celestial or terrestrial. The discovery of Newton, more especially when it was followed out by Halley, placed them in a different rank. Of unknown origin, but bearing in their appearance the marks of a foreign race, amenable to the great law of Nature, but setting at nought the customs (if I may so speak) of the other revolving bodies,—the gipsies, as I may term them, of the solar system,—the very singularity and strangeness of their motion seem to hold out the prospect of rendering to science some service, which the uniformity and similarity in the motions of the planets render them incapable of giving to us. And how are these wild bodies to be disciplined to our service? They are to be sent forth as spies; they are to go in directions in which no planets move; they are to explore spaces in which no other bodies are known to exist; and they are to return, bringing us an account, such as the physical astronomer can read, of the forces to which they have been subjected, and of the nature of the spaces through which they have passed. Have the anomalous motions of *Uranus* caused some astronomers to suspect the existence of a large planet beyond him? Then may we hope that Halley's or Olbers' comet will, in some revolution, feel its effect while far beyond our sight, and will return to our eyes still bearing, in its *disturbed motions*, a trace of the perturbations which it has undergone.

Has it for ages past been conjectured that some matter exists in the planetary spaces, which in time may sensibly affect the motions of the most dense bodies? Then will the comparative insignificance of the comets be more likely to feel its effects.

"I am speaking here, Gentlemen, of what may be hoped for the future; but in this I am fully borne out by the history of the past. There was a time, before the struggle of the rival theories of vortices and gravitation had terminated, when writers had learned to express the laws of gravitation in the language of vortices, and (in spite of the confusion of ideas which, when thoroughly examined, it was found to involve) to persuade themselves that the mutual attraction of many bodies could be explained by vortices. It was by the obvious impossibility of explaining the sun's attraction on comets moving in all directions, and to all distances, that the system of vortices was finally shaken, and the truth of gravitation established. And I do not hesitate to place in the same rank the discovery of resistance from the motions of Encke's comet. If it be objected that (so far as we can at present see) Halley's comet offers no trace of such resistance, a plausible ground of difference is not wanting. It is near the aphelion that the effects of resistance would be most sensible: the aphelion of Encke's comet is near the plane of the ecliptic, while that of Halley's comet is far from it; and if resisting matter be condensed (as the appearance of the zodiacal light leads us to imagine) near the plane of the ecliptic, then may the former of these comets experience considerable retardation, while the latter is almost unaffected by such a force. This, however, is precisely an instance of the points on which we may hope to derive information, from studying the motions of Halley's comet.

"These motions are to be studied by applying, with the utmost accuracy, the best theory that we possess, and by then examining the differences which may be found to exist between the motions thus computed and the motions observed. It cannot be too strongly urged upon you, that this is the only way in which new points of physical law have been made out, and that it is the only way in which even the mathematical theory of established physical laws has been made in any degree accurate. Every lunar or planetary irregularity of importance has been discovered by observation before it has been explained by the established theory of the day; and in many cases, nothing but a firm confidence in the accuracy of the comparison of the observations with a full developement of the previous imperfect theory, would have led the mathematician to extend his investigations so far as to form a more complete theory. The determination of the progression of the moon's apse, the history of the great inequality of *Jupiter* and *Saturn*, and that of the acceleration of the moon's motion, will sufficiently support me in this statement. With reference to the comet, the first step in the application of the theory must be, to investigate, from preceding observations, the elements of its orbit at the succeeding appearance; and the next step must be to render these elements immediately comparable with observation, by forming from them an ephemeris. And this leads me to make a few remarks on ephemerides in general. In this country, Gentlemen, it has been too much the custom to regard an ephemeris as a mere matter of convenience, serving only to assist in finding the body in the heavens. That this is one of its uses cannot be denied; but it is the lowest of its uses. To instance the most valuable periodical, with reference to planetary astronomy, that has been published, I mean our own *Meridian Ephemeris* (a publication of which this Society as a body, as well as various of its members as individuals, have just reason to be proud)—why is the right ascension of *Uranus* given to hundredths of seconds of time, when we know that it is four seconds in error? why is the polar distance of *Saturn* expressed to tenths of seconds of arc, when we know that it is incorrect to the amount of twenty seconds? The reason is, that, by calculating from the tables with the utmost accuracy (whatever the faults of these tables may be), we have from every observation an accurate comparison of the assumed theory with the observed fact; and we are thus in train for discovering the exact quantity of the correction which the elements require, or the exact amount of the unexplained inequalities (the *residual phenomenon*, to use the happy term of Sir John Herschel), for which new causes must be sought.

" To any one who is impressed with the spirit of the foregoing remarks, it will be evident that unity of method through the whole series of observations, from the very earliest that are considered trustworthy, to the latest that the time has allowed us to make, is quite indispensable. The same original elements must be employed through the whole. In some cases, as in the instance of Halley's comet at the late appearance, where a small alteration in some of the elements produced great and irregular changes in the apparent places, it may be desirable to use for comparison an ephemeris approaching as near as possible to observation; but as the elements adopted for such an ephemeris admit of immediate comparison with the elements as computed, this proceeding forms no departure from the general rule. It is desirable also, for the avoidance of irregularities in the mathematical part, that the same mode of calculation (as far as possible) be used throughout. I am happy to find the same ideas with respect to other bodies expressed so clearly in an addition to the *Berliner Jahrbuch* for 1838 (by Encke, I believe), that I cannot refrain from quoting them here: " For *Ceres*, *Pallas*, and *Juno*, it is absolutely necessary that a comprehensive investigation should be made; an investigation which may embrace all the observations extant, and which may not simply serve for the finding of these bodies, but may be considered as a firm foundation on which future investigations may be erected."

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"I have endeavoured, Gentlemen, to call your attention to the importance of observations of comets, on account of the singularity of their nature; the connexion which they seem to establish between the past and present states of our system, as well as between our system and the more sidereal bodies; the extent of the paths which they describe, and the unusual direction in which they move. I have pointed out that these observations cannot be made useful to the advancement of physical science, without a complete and accurate application of the theory which *we now* possess; and I have mentioned the advantage of unity of method *through the whole* extent of the computations. In a very few words I may point out how far M. Rosenberger has satisfied these demands. He has used the theory of perturbations in its most accurate form, and in one of its most delicate parts he has introduced an important correction. He has computed the perturbations of all the elements, for the first as well as the second revolution; Damoiseau and Pontécoulant having only computed the change of mean motion and epoch for the first revolution. He has included in his calculations the effect of several planets which had been totally omitted by other mathematicians. He has not only computed the perturbations, but has also, from the observations of different astronomers at the former appearances, investigated the elements at those times, by a process of the most accurate kind. So complete are the whole of these computations, that if names were taken, not from the discoverers of these bodies, or from those who conjecture their identity, but from those who, by accurate observations on a uniform system, combine the whole of our information relating to them, we should call this body, not Halley's, but Rosenberger's comet[*]. I may add that, in the various memoirs to which I have referred, the theory connected with the computations is so well stated, that the study of these papers will be found most useful to the future investigator. I trust, Gentlemen, that it is unnecessary for me to say another word in defence of the adjudication of this year's medal.

(The President then, addressing the Foreign Secretary, continued thus:—)

"Captain Smyth,—Transmit this medal, on the part of the Royal Astronomical Society of London, to M. Rosenberger. Assure him that his skill and his labour, though not imitated, are appreciated, in this country. Say to him, that we trust he still retains the intention of extending his calculations to periods preceding and following those which he has already discussed. And convey our best wishes for his health and happiness, for the enjoyment of his well-deserved reputation, and for the vigour which may enable him to extend it by other investigations, as delicate as that already made, on other subjects of equal importance in the system of the universe."

[[*] This remark is precisely in the spirit with which M. Arago, we think most justly, claims for the comet of Hr. Biela, the appellation of "GAMBART," (see *Magazine of Popular Science*, vol. ii., p. 283). Mr. Airy, however, has here made a proposal demanding a voluntary transfer of national glory, which we believe but very few Frenchmen, and last of all M. Arago, would ever have dreamed of suggesting.—EDIT.]

MISCELLANEA.

November-Asteroïds.

A **sharp** look-out appears to have been kept at Plymouth, both at sea, and on shore, for the shooting-stars which were expected on the night of the 12th—13th of November last*. Several officers of the navy, and Mr. Southwood, of Devonport, have enabled a committee of the Plymouth Institution to transmit a diagram of the meteors observed, accompanied with notes of the time of each phenomenon, its altitude and direction, the character of the light, and incidental remarks.

On Nov. 11, there was noted	1
— 12, weather hazy	0
— 13,	25
— 14,	18
— 15,	16
Total	60

The directions were,

West	31
South-west	3
North-west	1
North	6
North-east	1
South	4
South-east	2
South-east by East	1
East	2
Unknown	9
Total	60

The times were,—

Between sunset and midnight	19
Between midnight and sunrise	41
Total	60

The Constellations in which they appeared, were,—

Cygnus	1	Cassiopeia	2
Musca Borealis	1	Cepheus	1
Auriga	12	Pegasus	2
Lynx	2	Orion	2
Perseus	5	Monoceros	1
Camelopardalus	1	Gemini	3
Triangula	2	Draco	1
Ursa major	1	Cancer	1
Ursa minor	1	Cetus	1
Leo minor	1	Canis major	1
Andromeda	2	Virgo	2
Taurus	14		

Rules for determining the comparative Merits of Microscopes.

"The Judges.

"It will be necessary to appoint two indifferent persons as judges; and if they cannot agree, to refer the matter to a third, as umpire, whose judgment should be final, and without appeal. If it should be asked, of what description should such persons be—should they be profound opticians or microscopists?—I should answer, that any individuals whose sight is perfect are competent to the task; the more so, perhaps, if they never looked into a microscope in their lives, for then they are likely to be without prejudices of any kind. I was myself, when I first began to reform microscopes, very much in the habit of taking the opinion of *an actress* on them; and her decisions were anything but agreeable to me, though I believe they were perfectly correct. Whatever objections there may be to the owners or inventors of particular instruments being allowed to have any voice in determining their specific merits, they are the most proper persons in the world to have the management of them, as they will be sure to display their powers and properties to the greatest advantage, and make them so put forth their whole mettle as certainly as a man will who rides his own horse, and has betted money in his favour on the race-course: therefore it is not necessary for the judges to understand a tittle of the method of managing microscopes or engiscopes, all of which is to be left to the commanders of them: the said judges have nothing to do but to look at the object when duly prepared for their scrutiny.

"Having pitched upon judges, it remains for us to ascertain by what laws they shall be governed in their decisions; for it is impossible to give a sound judgment in any case without reference to certain principles; even a common game at cards cannot be lost or won but according to certain rules.

"I shall lay down what I consider the laws by which microscopes and engi-

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See the article "November-Asteroïds," p. 56.

scopes ought to be judged beaten or victorious; they who do not relish them can establish others, which they may consider more equitable; for laws there must be, of some kind or other, before we can proceed a step in our decisions.

"THE CODE.

"*Art.* 1.—That instrument is the best which shows the different details of objects most decidedly, and with the greatest clearness and perspicuity*; no matter of what nature or kind, or whether it is chromatic or achromatic, planatic or aplanatic, in or out of adjustment, or whether its lenses are well or ill-worked, or polished, or centred. If a microscope made out of the lens in the eye of a stinking whiting *would show me something which I could not see with any other*, I should say it was the best, and had beaten everything else out of the field.

"*Art.* 2.—*The same identical object* must be applied to each microscope and engiscope, tried against each other: supposing the objects to be the scales of insects, a drawing must be made of the configuration of them, that we may be able always to pitch upon one particular specimen.

"*Art.* 3.—Cæteris paribus, I should say, *that instrument which will show an object perfectly with the lowest power is the best:* thus if one instrument, A, shows an object distinctly and satisfactorily with a power of 200, while another, B, will show all its minutiæ equally well, though *on a smaller scale*, with 100, I should say that B was the best: in this case I suppose that when the power of each is made equal to 100, the performance of both is not equal, but that B has the advantage. But if the power of each was raised to 200, and then A had the advantage, I should still say that B was the best. I have often insisted on this point in my writings, and assigned what I consider sufficient reasons for my assertions.

"*Art.* 4.—If two instruments, C and D, show the lines and markings on an object equally well; but C shows the edge of the scale or feather with the same adjustment of the focus which serves best to bring out the lines, so that the outline and the lines are simultaneously visible, and D does not; then C is the best.

* "The capability of counting accurately the number of any particular traits or objects, developed both in telescopes and microscopes, has been adduced as decided evidence of the goodness of these instruments, and their power of showing things in a *decided and forcible manner*. I know not exactly how the truth may be; of this I am certain, that I have seen objects as I should say with the very *intensiva of distinctness*, and yet I am quite confident I could not have counted them truly: perhaps I have not acquired the habit of doing so, or may have some natural incapacity on the subject; for I have frequently found myself unable to count aright the number of sheep in a meadow, when not amounting to more than three-score. I have heard graziers assert that considerable practice is necessary in order to be able to count sheep and herds of cattle accurately.

"Lined objects are seen to the greatest advantage, and in the most perfect style, when the spaces between the lines are clear, and the lines themselves dark and strong, as if drawn with a pen and ink on white paper. When scales and feathers are exhibited as opaque bodies—for example, a scale of the diamond beetle—with a high power, the lenses should appear as furrows, or with distinct ridges, dark one side and light on the other, as a ploughed field seen when the sun is low in the horizon, and its *beams play* across the furrows. Where several systems of faint lines cross each other *in an irregular manner*, the result will be an appearance similar to the watering of silks and moreens. This may be verified by applying two wire sieves, or two pieces of worsted gauze, to each other, and holding them up to the light. The markings on some scales of the podura seem to be of this description; others seem mere points, arranged so as at first sight to give the appearance of a system of lines, or of two crossing each other. The studs on the skin of a boiled pullet, seen as opaque bodies, removed to some distance from the eye, are not an inapt illustration of this sort of optical deception; those of the feathers near the pinions, in particular.

"There is certainly a very great difference in the degree of facility with which different specimens of the *podura* may be resolved into lines, as well as in the strength and plainness of their markings. A curious phenomenon sometimes presents itself in some choice pet scales, having straight lines from end to end, and two systems of oblique lines also, both apparently grooved in a decided manner, as is the case with many I have seen; namely, one of these systems may be seen by looking *directly*, and the other, *without any alteration in the illumination*, by looking *obliquely* into the instrument. The same circumstance occurs with some other tests."

"*Art.* 5.—If one instrument shows the lines on any particular test as if composed of an aggregation of dots or globules; or exhibits them broken, interrupted, and ragged, while another shows them clearly made out, as veritable lines or stripes drawn with a pen and ink; the latter is the best. In the first paper I wrote on test objects, I have given as many as five degrees of illusive or false vision produced in the appearance of the feathers of the *Morpho Meneläus*, by viewing them with an object-glass deficient in defining and penetrating power.—Vide *Quarterly Journal*, vol. xxii., p. 265.

"*Art.* 6.—If two instruments show certain lined objects *as transparent bodies* equally well in all respects, but one show them more or less evidently *as opaque objects* also, while the other will not, then it has clearly the advantage over the other.

"N.B.—One of the best methods of exhibiting scales as opaque bodies is to take away the disc of talc next the eye, replacing the ring; and then to attach a black wafer, or a bit of black paper, to the reverse of the remaining piece of talc, to which some of the scales will generally adhere. In this case I have supposed the scales under consideration to have been mounted in the ordinary way, in slides or circlets, as transparent bodies; and that we wish to be sure of seeing some of the same scales we have seen as transparent bodies, as opaque ones also. If the upper piece of talc is allowed to remain, it will prove a considerable detriment to vision; but still it will be as unfavourable to one instrument as to another.

"*Art.* 7.—If two instruments should prove equal in all other respects, but one is achromatic and the other not, that which is achromatic has of course the advantage, as it will show the objects perfectly free from false colouring.

"*Art.* 8.—If two instruments seem to show transparent bodies equally well, but one of them, when tried upon opaque ones, has a slight fog of the diffused kind over its whole field, or a penumbra or nebulosity encircling numerous points, to a certain distance from them, while the other is free from this illusion, that which shows the opaque objects best is the winner, for opaque objects are still more severe *tests than transparent ones.*

"*Art.* 9.—Whenever one instrument happens to show some marking or feature in an object not visible in another, and the owner of the latter attempts to get rid of this, by asserting that it is an illusion produced by some defect of his antagonist's instrument, let him be made to *prove* his assertion to the satisfaction of the judges, or the umpire; and if he cannot do so, let him be turned adrift as a scamp and blackleg. Let us suppose, for example, that the trial was the wheel animalcule *(vorticella rotatoria)*; and that in the instrument said to exhibit an illusion two points were visible near the head of the animal, when in its grub form, with the wheels withdrawn, (supposed by some naturalists to be the eyes of the animalcule) I should say that the *illusion* here was in the instrument which *did not show them;* for a false evidence may be given by suppressing facts, as well as by coining false ones.

"There are several minor points which might be insisted upon; such as that instruments which have a strong natural light, combined with a large field of view, free from distortion, and equally good all over, have the advantage over such as cannot pretend to these properties; but as such instruments do not seem to possess any absolute power of *showing anything more or better* than others, I shall lay small stress on these properties."—GORING. *Micrographia*, 1837.

Flowers artificially injected.

In presenting to the *Académie des Sciences* on the 30th of January last, some flowers of the white hyacinth injected and rendered red by absorption of the juice of the *Phytolaca decandra*, (Virginian poke,) M. Biot observed, "Naturalists frequently feel under the necessity of introducing into vegetable tissues coloured liquors, in order that, by their presence and distribution, the minute texture, as well as the course, of the alimentary vessels of the tissue, may be indicated. But the greater part of the colouring-matters are absolutely rejected by these tissues, or they penetrate them with difficulty, and therefore cannot be carried far enough; or finally, they act upon them, and alter their nature.

"There may be found in the *Recueil des Prix de l'Académie de Bordeaux*, t. iv., a curious memoir on the motion of sap, in which M. Delabaisse, the author, states, that the juice of the *Phytolaca decandra* is free from these objections, and that with it he had perfectly succeeded in injecting of a red colour, by absorption, all kinds of white flowers, and even of green leaves. But at the period of this memoir (1733), physiology and vegetable chemistry were too little advanced to enable the author to derive from his discovery all its possible advantages, or even its necessary consequences; and although his experiments have been quoted in several general treatises, I am not aware that they have hitherto been verified or carried further.

"At a time that I was studying the motion of sap, they occurred to my memory; and having raised several plants of *Phytolaca*, I have lately employed the juice of it to repeat them. Now, although I have generally succeeded in the process, I found it attended by many difficulties that the author does not point out, and which deserve to be attentively studied. Many plants absolutely refuse the injection; others take it rapidly, without the place of any in the natural system suggesting a reason for the difference. A few minutes have been sufficient to streak with a multitude of delicate red lines, all the petals of a monthly white rose; while a musk-rose, equally white, was not at all affected. Further, flowers of the same kind, taken from the same individual, show similar discordances with the same liquor. Finally, when the injection does take place, I believe it possible to ascertain its progress, its several appearances, the points where it will accumulate, and where it ought first to appear, but that very differently from the way supposed by the discoverer of the process. But the apparent caprices remain still to be analyzed, in order to make a general application of them.

"I shall prepare myself during the next Autumn, for the purpose of giving, if I can, a little more generality to the process, by the employ of the numerous phenomena of colouration which modern chemistry affords. In the mean time, I have thought that a simple confirmation of the fact observed by Delabaisse might be useful to those naturalists who are interested in its use; and that giving them information of it sufficiently early, might induce them to raise some plants of the *Phytolaca* for this purpose, in the ensuing Summer."

New Compound of Hydrogen and Carbon.

"I HAVE ascertained that a compound of carbon and hydrogen, not previously noticed, certainly exists; this compound consists of 1 atom of hydrogen = 1, and 1 atom of carbon = 6, so that its atomic weight is 7; and it therefore differs in composition from all other compounds of these two bodies already known. I propose to call it *protohydruret*, or *protohydroguret* of carbon. Nothing is, perhaps, more difficult to remember than the various names which have been given, from time to time, to the previously known compounds of carbon and hydrogen, and these names I suspect will now require to be changed. I will give an early account of the properties, as well as the mode of preparing the new compound in question. I may observe, *en passant*, that the new substance differs entirely from olefiant gas, which is composed of double the quantities of carbon and hydrogen above enumerated, and consequently having 14 as its atomic weight."—W. MAUGHAM, *Lecturer on Chemistry, Adelaide Street Gallery, in Letter to* EDITOR, *dated March 20th*, 1837.

Reluctant but important Encouragement of Scientific Knowledge.

IT is with great pleasure that we extract and give increased circulation to the following introductory paragraph in a Catalogue of Anatomical Preparations in Wax, recently issued by Mr. Schloss, 2, Great Russell Street, Bloomsbury Square.

"The encouragement Mr. Schloss has received from the Scientific Institutions of Great Britain, has induced him to make application to the Lords of His Majesty's Treasury, for a remission of the duty on such Anatomical Models in Wax as are imported for public establishments; and it is with great pleasure he now has the honour to inform the profession, that the prayer of his petition has lately been granted by their Lordships, and that such models may now be obtained at a reduction of twenty-five per cent."

But we cannot help adding, that some portion of the indignation felt at the ignorant daring of legislators, whose Custom-House regulations prevented the introduction into their country of preparations so highly important to humanity, must still burn against the "Lords of His Majesty's Treasury" who have remitted the duty on such anatomical models as are imported for *public establishments only*. Instead of restricting by a tax, they ought to encourage by a bounty, the most extensive circulation of such extremely useful, and at the same time, exquisitely beautiful, objects. What is their ultimate effect? Is it not the alleviation and prevention of human suffering? And yet upon the introduction of such precious means into the study of the private practitioner do the "Lords of His Majesty's Treasury" still presume to continue "let and hindrance"!

Prof. Bessel's Description of the Solar Eclipse of 1836.

PROF. BESSEL, esteemed one of the best living astronomers, has published the following description of the solar eclipse of May 15, 1836. The observations were made at Königsberg, in Prussia. At p. 151 we gave the observations of the same phenomenon by Mr. Baily, in which some very remarkable appearances are detailed. None such are mentioned by Prof. Bessel. The latitudes of the two observers were not very widely different, but still they varied so much, that the annulus was never completed to Prof. Bessel's view, and this may be the reason of the discrepancy in the appearances.

"The observatory of Königsberg is situated so near the northern limit of that zone of the earth's surface within which the eclipse appeared annular, that Mr. Bessel could not determine with certainty, from the previous calculations, whether the limit extended to the observatory, or went beyond it. Encke's *Ephemeris* gave the eclipse annular for the observatory, but it gave the annulus at its narrowest part a breadth of less than 4″, which is within the limits of the probable errors of the tables in respect of the moon's latitude. It was doubtful, therefore, whether the eclipse would appear annular; but, in any case, the near proximity of the observatory to the annular limit gave the observation an unusual interest, inasmuch as an opportunity of observing an eclipse under similar circumstances must be of very rare occurrence.

"Mr. Bessel observed the eclipse with a power of 179 applied to the heliometer. *During its continuance,* the part of the moon's border on the sun exhibited, as usual, protuberances and cavities. The exterior edges of the cusps, which, at the time of the nearest approach of the centres, were extremely fine, had the regular curvature of the sun's border, but their interior edges showed the irregularities of the border of the moon. They appeared, also, when their extremities fell on a part of the moon's limb not particularly mountainous, to terminate in sharp, prominent points; whereas, when the extremities fell on the slope of a lunar mountain, they assumed a corresponding figure. There was no rounding of the points which could not manifestly be ascribed to this circumstance; nor was anything seen in the telescope made use of, which could be considered as indicating an irradiation of the sun's disc.

"At the time the cusps approached nearest to each other, they terminated at a very rugged part of the moon's limb; and Mr. Bessel expected, but in vain, that some parts of the sun's disc would make their appearance between the points. About twenty-five seconds, however, before the nearest approach, there appeared, near the termination of the upper cusp, a point, which, though far, indeed, from exhibiting the clear light of the sun, was sufficiently distinguishable in the powerful telescope of the heliometer. As the cusps had then approached very near, he expected every instant to see the annulus formed; this, however, did not take place, but the luminous point became more bright, and other similar points appeared besides it, which soon united, and in this manner rendered visible the whole of the moon's border, between the extremities of the cusps. Still he looked for the formation of the annulus itself; and he first remarked it would not take place, when the disappearance of some points on the moon's border showed that the eclipse had begun to decrease.

"During the time that the moon's limb continued visible, he could clearly distinguish the hills and valleys. The brightness of the moon's limb might have about the same proportion to the brightness of the sun, as the brightness of that part of the moon's disc which is illuminated by the earth a short time before or after the change, has to the brightness of the portion illuminated by the sun. According to this estimate, the illumination of the visible part of the moon's border was so strong, that it might be supposed it could scarcely escape notice; yet it was not perceived by three other observers, Mr. Busch, Mr. Zernow, and William Bessel. When the cusps approached nearest each other, he estimated their distance to be four minutes at most. It would have been easy to have measured it, but he was apprehensive that, by so doing, he should hazard the observation of the duration of the annulus, which he expected every instant to appear.

"The time during which the whole portion of the moon's limb between the cusps was seen, was perhaps fifteen seconds; some points of it were visible longer. The depressed parts of the border appeared in a stronger light than the protuberant parts. This light was much feebler than that of the sun's disc, but much stronger than the usual light of the moon, being, in fact, visible through the coloured glasses of the telescope, which (as he afterwards ascertained by direct experiment) the ordinary moonlight could not penetrate, even when the illumination of the atmosphere was much feebler than it was during the eclipse.

"Mr. Bessel considers the phenomenon observed by Mr. Van Swinden at Amsterdam, on the occasion of the solar eclipse of the 7th of September, 1820, of which a description is given in the 1st vol. of the *Memoirs of the Astronomical Society*, to have been the same as that which he observed.

"This eclipse was annular at Amsterdam; but the duration of the annulus was only about three-quarters of a minute, so that the approach of the two borders before the formation of the annulus, and their separation after it was broken, took place very slowly, and Mr. Van Swinden was consequently enabled to see what preceded and followed these instants (while the borders were almost in contact), during a much longer time than can usually be done in the case of annular eclipses, in which the phenomenon, on account of its almost instantaneous disappearance, cannot, perhaps, be observed with sufficient certainty. Mr. Van Swinden also remarked a bright arch connecting the two cusps, but he makes no mention of the inequalities of the moon's border, which were seen on it by Mr. Bessel. As the duration of the phenomenon at Amsterdam, *notwithstanding* the favourable circumstances, was extremely short, Mr. Bessel thinks it probable that it was on this account that Mr. Van Swinden failed to notice all the appearances which presented themselves to him in Königsberg, under circumstances still more favourable, by reason of the greater length of time the phenomenon continued. He thinks, therefore, that the circumstance of Mr. Van Swinden's not mentioning the inequalities of the moon's limb, affords no ground for calling into question the identity of the appearances. There is also another discrepancy between the two descriptions. The luminous arch connecting the cusps is described by Mr. Van Swinden as having a smaller radius of curvature than the moon. Mr. Bessel, however, thinks this was only an optical deception, occasioned by its having a different curvature from the sun.

"From the estimate given above of the brightness of the line connecting the cusps, it follows that it was much too bright to be ascribable to the usual reflected light of the sun, far less to any lunar twilight, or to any inflexion of the solar rays. He, therefore, thinks it a probable hypothesis, that the sun itself is surrounded by a corona of luminous matter, which is not covered by the moon's disc, while the latter covers the disk of the sun. Such a corona must have a very small extension, inasmuch as it only becomes visible when the moon *nearly* covers the solar disc. That it cannot be remarked about the border of the sun, when uncovered, may be explained, partly by its extremely small breadth, and partly by the feebleness of its light, in comparison of the light of the sun."

Salutary Abhorrence of the "Rule of Thumb."

"LET the young mechanist allow an old engineer to give him a few parting words of advice, derived from long observation and experience in wheelwork. Above all things, let him abhor the 'rule of thumb;' and let him always be able to give a reason for his practice. Let him not imagine that a thing is right, because such and such great houses adopt it, for it often happens, that the foreman, who has the direction in this department, has no other qualification than that of being a good and careful workman, which he may be, and yet be totally unacquainted with the principles upon which he ought to proceed. He produces, perhaps, a passable result, because it was his good luck to copy a passably-good practice in his youth.

"Let *him*, however, who would go to work with an understanding of his subject, investigate for himself, and take nothing upon trust; but let him ascertain for himself, the truth of every proposition he admits, and not blindly follow the practice, or submit to the judgment of others."—Hawkins's Camus *on the Teeth of Wheels.* Second Edition, 1837.

Variation of the Magnetic Needle at London. Dec. 1836.

If inquiry be made for the accurate amount of the present variation of the magnetic needle at London, it is remarkable how difficult it is to obtain anything like a satisfactory answer. A valuable correspondent has enabled us to make the following communication.

"On December the 14th last, I found, by equal altitudes, with a good instrument, that the variation was 23° 37′ 30″ West.

"*Fleet-street,* Feb. 25, 1837."

The Crosse-Insects.

"I should wish to observe that, with respect to those experiments of mine in which insects made their unexpected appearance, I have given no opinion whatever as to the cause of their production, having, as I at first stated, mentioned 'facts, but not opinions.' Without more data than we at present possess, I do not see that it is possible to form an opinion on the matter, or to say whether the electric agency is or is not a secondary cause, or acceleration of their birth.

"Since my two first experiments, I have met with eight other results in which similar insects have appeared; in the whole, ten separate formations. Four of these have been in siliceous solutions, and four in other fluids, one of them a concentrated solution of nitrate of copper. In all of these the electrical agency was long continued before the insect made its first appearance, but this might have been the case otherwise.

"In the course of my observations *I have met with* some rather curious phenomena, which shall be laid before the public when the train of experiments now in hand, and which must necessarily occupy much time, is completed." —Mr. Andrew Crosse, *to Editor of Times, March* 16, 1837.

"These insects were first supposed to be *Infusoria*, similar to those discovered by the microscopic observations of Ehrenberg, but they are now considered by naturalists in London to belong to a higher order, resembling very closely the well-known *Acari*, which infest cabinets."—Prof. Buckland. *Communication to the Ashmolean Society,* March 2, 1837.

Faith injurious to (mechanical) Works.

In 1803, a republication of Imison's School of Arts, under the title of Imison's *Elements of Science and Art*, was given to the world by Mr. T. Gill. In it he inserted some practical directions for the formation of the teeth of wheels. In the editions of Ferguson's *Lectures on Select Subjects*, which appeared shortly afterwards by Mr. (now Sir David) Brewster, these were condemned as being "so defective in principle, that they cannot be trusted. The author seems merely to have heard that the acting faces should be epicycloidal, but to have been totally ignorant whether the epicycloids should be exterior or interior, and what should be their bases and generating circles."—"It is demonstrable and evident to every person who understands the subject, that the generating circles with which he describes his epicycloids, are *twice as large* as they ought to be."

In addition to this authority of Mr. Brewster, who had reconsidered the subject in the interval of the publication of the two editions of Ferguson, and given our last quotation in the second, may be added those of Camus, Dr. T. Young, Buchanan, Peter Nicholson, Reid, and every other author who had written on the subject, except Mr. T. Gill, the editor of Imison's Elements; "all besides him, having irrefutably proved that the epicycloidal part of a tooth, designed to act on another wheel or pinion, against a part of a tooth lying in a plane cutting the two axes, must, to ensure smooth and durable action, be generated by a circle equal to the radius of the wheel or pinion, with which it is to be engaged; and not equal to the diameter, as contended for by the editor of Imison's Elements, and unfortunately acted on by many of our most eminent engine-manufacturers, some of whom have confessed that they adopted the erroneous practice without investigation, from faith in the very extensive mechanical knowledge possessed by the promulgator of the error.

"The editor appreciates that knowledge so highly, that he has been in the habit, for more than thirty years, of designating his friend a walking encyclopædia; and has referred to him hundreds of times for information, as to what has been discovered and performed in numerous branches of science and art. His personal regard, therefore, would have prompted him to screen the editor of Imison's Elements from the exposure of his error, could he have forgotten the public duty which devolves upon him as editor of Camus; that duty imperiously calling on him to display the truth of the subject treated on, in the fullest and clearest manner, irrespective of the feelings or prejudices of any person whomsoever*."

The difference of opinion appears to have been pertinaciously adhered to on both sides for more than thirty years; and Mr. Hawkins, in his new edition of Camus, states some curious facts relating to the great prevalence of the erroneous doctrine. It is not a little remarkable that most of our practical men, in the face of the greater and more numerous authorities, appear to have adopted the "false notions" of Mr. Gill. "They have obtained," says Mr. Hawkins, "an extensive acquiescence on the part of many of our first-rate engine-manufacturers, some of whom are pouring into the market multitudes of cast-iron wheels and pinions of various magnitudes, for cotton, and other machinery, with teeth formed from the epicycloid of the diameter, instead of the radius, of the opposite wheel, or of the opposite pinion." And "in the hope of awakening those manufacturers† to a sense of the injuries which they are occasioning to their customers, by supplying them with wheel-work that must wear out in a few years, instead of lasting the greater part of a century, which many of the wheels would do, were the teeth formed on true principles." Mr. Hawkins ha quoted some sensible remarks, (taken from Rees's *Cyclopædia*, article "Clock Movement,") said to have been made by Mr. Thomas Reid, an eminent clock-maker of Edinburgh.

Mr. Hawkins has also, in the true spirit of a lover of excellence, hunted out these "false notions" in the recesses of the workshop, and gives the results of his inquiries in a section, which he entitles PRESENT PRACTICE, but which might also be as justly headed "Present Blundering." We give the greater part of it, in the laudable intention of assisting in the "awakening" of *all* parties concerned; certain that if the customers will bestir themselves, the real or affected sleep of the machine makers would soon terminate.

"PRESENT PRACTICE.

"Before stating the present practice in forming the teeth of wheels, the editor takes much pleasure in acknowledging the very polite attention he has received from a considerable number of the principal manufacturers of machinery, in which toothed wheels are used; many of whom have permitted him to question their foremen, pattern-makers, and workmen, and to inspect the means, instruments, and tools used by them respectively.

"Among the houses especially entitled to his thanks for liberally affording him every facility in his inquiries, are the following:—Messrs. Maudslay and Field, Engineers, London; Rennie and Co., ditto; Donkin and Co., ditto; Bramah and Co., ditto; Seaward and Co., ditto; Mr. Clement, ditto; Mr. Topham, ditto; Messrs. Sharp, Roberts, and Co., ditto, Manchester; Guest, Lewis, and Co., Dowlais Iron-Works, Merthyr Tydvil; Troughton and Simms, Mathematical Instrument Makers, London; Arnold and Dent, Chronometer Makers, ditto; Mr. Vulliamy, ditto; Mr. Saxton, Mechanician, ditto; Mr. Fayrer, Philosophical Instrument Maker, ditto.

"A painful task now presents itself, which the editor would gladly avoid, if he could do so without a dereliction of duty, namely, to declare that there is a lamentable deficiency of the knowledge of principles, and of correct practice, in a majority of those most respectable houses in forming the teeth of their wheel-work.

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* Hawkins's Camus *On the Teeth of Wheels*, Second Edition, 1837.

† Some of these gentlemen may be asleep through ignorance, as Mr. H. charitably observes, but it is more than probable that some of the rogues are "wide awake" to the profitable consequences of their *rule of thumb.*

"Some of the engineers and millwrights said, that they followed Camus, and formed their teeth from the epicycloid derived from the diameter of the opposite wheel; and that they were induced to do so by the recommendation of the Editor of Imison's Elements. It is clear, therefore, that they could not have read Camus with due attention, or they could not have imagined that they were working on his principles, while they were depending only on the false view given in the paper prefixed to Camus, already so much animadverted on.

"One said, 'We have no method but the rule of thumb;' another, 'We thumb out the figure;' by both which expressions may be understood, that they left their workmen to take their own course.

"Some set one point of a pair of compasses in the centre of a tooth, at the primitive circle, and with the other point describe a segment of a circle for the off-side of the next tooth. Others set the point of the compasses at different distances from the centre of the tooth, nearer or farther off; also within or without the line of centres, each according to some inexplicable notion received from his grandfather, or picked up by chance. It is said inexplicable, because no tooth bounded at the sides by segments of circles, can work together without such friction as will cause an unnecessary wearing away.

"It is admitted, that with a certain number of teeth, of a certain proportionate length as compared with the radii, there may be a segment of a circle drawn from some centre, which would give 'very near' a true figure to the tooth; but 'very near' ought to be expunged from the vocabulary of Engineers and Millwrights; for that 'very near' will depend on the chance of hitting the right centre and right radius, according to the diameter of the wheel, and the number of teeth; against which hitting the odds are very great indeed.

"Among the Mathematical Instrument Makers, Chronometer, Clock and Watch Makers, the answers to the inquiries, were, by some, 'We have no rule but the eye in the formation of the teeth of our wheels;' by others, 'We draw the tooth correctly on a large scale to assist the eye in judging of the figure of the small teeth;' by another, 'In Lancashire they make the teeth of watch-wheels of what is called the bay-leaf pattern; they are formed altogether by the eye of the workman: and they would stare at you for a simpleton, to hear you talk about the epicycloidal curve.' Again, 'The Astronomical Instrument Makers hold the bay-leaf pattern to be too pointed a form for smooth action; they make the end of the tooth more rounding than the figure of the bay-leaf."

"It is curious to observe with what accuracy the practised eye will determine forms; it is not generally known, but it is a fact, that the dots marking the minutes on the enamelled dial-plates of watches, are put on by hand with a camel's-hair pencil guided only by the eye; they are done with great expedition, and rarely can an error in their distances be measured.

"Such being the accuracy of the eye, how important it is, that these Lancashire bay-leaf fanciers should be furnished with pattern-teeth of large dimensions, cut accurately in metal, or at least in card-board; and that they should frequently study them, and compare their work with the patterns. These Lancashire workmen are called bay-leaf fanciers, because they cannot be bay-leaf copiers, since it is notorious that there are not two bay-leaves of the same figure.

"Mr. Saxton, of Philadelphia, now in London, who is justly celebrated for his excessively acute feeling of the nature and value of accuracy in mechanism, and who is reputed not to be excelled by any man in Europe or America for exquisite nicety of workmanship, made in Philadelphia an instrument for cutting the teeth of watch-wheels truly epicycloidal, or rather for curving them, after they were cut down in the ordinary manner, with radial faces. The following is his verbal description of this instrument.

"The wheel to be rounded being put on a vertical arbor, another arbor stood parallel to the first, carrying, on a third but horizontal arbor, a steel wheel-file, cut on the plane side, which plane side lies in a vertical plane, passing through the axis of its vertical arbor. On the arbor of the wheel to be rounded is a circular plate, equal in diameter to the primitive circle of that wheel; the edge of the plate is milled into teeth as fine as possible: this plate forms the base of the epicycloid. On the other vertical arbor is a similar plate, but equal in diameter to the radius of the primitive circle of the wheel to be engaged with that about to be rounded: this plate is the generating circle.

"In working this instrument, the flat-sided cutter is brought in contact with the side of the tooth to be rounded; the axes of the two vertical arbors, the face of the cutter, and the line of the tooth, all lying in one vertical plane; the cutter being set in rapid motion by a band, the generating circle is rolled around the base, and thus one side of the tooth is rounded in a truly epicycloidal curve of the required dimensions. Upon this plan, epicycloidal teeth of any magnitude might be cut with great expedition.

"Such an instrument ought to be added to every engineer's stock of tools.

"The application of an engine of this kind in forming the teeth of chronometer-wheels would be of very great importance; and scarce any pains that could be bestowed in perfecting the figure of the teeth of wheels for measuring time truly would go unrewarded.

"One of our most eminent watchmakers, however, says, that the prices at which even first-rate watches are sold, will not warrant the care that must be bestowed on them, to insure perfect accuracy in the figure of the teeth of all the wheels of a watch."—HAWKINS'S CAMUS *on the Teeth of Wheels.* Second Edition, 1837.

Hope deferred. Second Series.

IT would seem that the same disheartening series of "deferrings" is commenced with regard to the Patents for Inventions' Bill of this session, as were so vexatious and fatal in the last. The bill was introduced in the House on the 15th ult., and read a first time, and ordered to be read a second time on Wednesday, the 1st of March.

The following is a copy from the Report of the business done in the House of Commons on Wednesday, the 1st of March:—

"Patents for Inventions Bill, 2° Reading *deferred* till *Wednesday,* 12th April."

One would think that even Mr. Nicholls had ventured to be sarcastic upon his masters, in thus printing the word *deferred!*

Captain Smyth elected a Member of the Institute of France.

ON the 13th of February last, the *Académie des Sciences* received from the Astronomical Section, by the hands of M. Mathieu, the following list of persons eligible, in the opinion of the Section, as corresponding members of the Academy:—

1. Smyth . . .	*Bedford.*
2. Littrow . . .	*Vienna.*
3. Hansen . . .	*Gotha.*
4. Santini . . .	*Padua.*

The election took place on the 20th of the same month. Thirty-nine members voted, in the following manner:—

Smyth . . .	30
Littrow . . .	7
Hansen . . .	2

Captain Smyth was therefore elected, and is the fourth Englishman who has obtained this honour in four months.

Present Amount of Steam-power in Birmingham.

THE number of steam-engines actually working at the present time in Birmingham, is *one hundred and sixty-nine,* amounting in the aggregate to *two thousand seven hundred* horses-power. They are distributed as follows:—

	Horses-power.
In grinding flour	275
— working metals	1770
— pumping water	279
— glass-grinding	87
— working wood	97
— making and glazing paper . .	44
— grinding clay	37
— grinding colours and chemicals	61
— sundries	50
	2700

BIRMINGHAM PHILOSOPHICAL INSTITUTION. *Report of Committee,* 1836.

Fribourg Suspension-Bridge.

IN this Magazine, vol. ii., p. 332, some anxiety was expressed for the safety of the new Suspension-Bridge of Fribourg, in consequence of the representations of an American gentleman. We are happy to say that we believe the alarm was utterly groundless. A member of the Civil Engineers stated to that Institution in a recent meeting, that he had visited the bridge in October last, had passed it in a carriage, and, during his examinations and inquiries, neither saw nor heard anything that induced a suspicion of its stability.

Patent-Law Grievance. No. XIII.

The inventors of this country, and the introducers of inventions of other countries into this, were obliged to pay down to the attorney-general and other agents, &c., of the Government during the past year, above £42,000. *What did the attorney-general, &c., in return to them for this vast and oppressive extortion?*

If we look a little further back, the following exhibition of this legal wrong will then present itself.

Total Sums paid by Inventors, in Ten Years, ending December, 1834, according to the Returns made, and calculations of extras and occasional charges.

	£	s.	d.		Patents		£
English patents, per report	106	11	8				
Stamp duty on specification, average	8	0	0				
Official charge of drawing, do. do.	5	5	0				
Extras; joint names, private seals, journeys, expedition, caveats, attendance in opposition, &c. &c.; *exclusive* of drawings and extra charges of the patent solicitor	12	3	4				
	132	0	0	×	1,684	=	222,288
Stamp Duty on Warrants, Colonies	6	0	0	×	1,684	=	10,104
Scotch patents, per report	79	10	5				
Stamp duty on specifications, average	8	0	0				
Charge of drawing, do. do.	5	5	0				
Extras, joint names, &c. &c.	7	4	7				
	100	0	0	×	489	=	48,900
Irish patents, per report	128	5	11				
Stamp duty on specification, average	4	0	0				
Drawing, do. do.	5	5	0				
Extras, joint names, &c. &c.	7	9	1				
	145	0	0	×	207	=	30,015
Colonies, at additional charge with extras, average	7	10	0	×	300	=	2,240
Total amount paid by inventors in ten years, on 2380 patents							£313,557

Mackinnon. *Speech in the House of Commons, Feb. 14, 1837, Appendix.*

Three hundred and thirteen thousand, five hundred and fifty-seven pounds, "paid by inventors in ten years," *exclusive*, it will be observed, of the drawings and charges of the patent-solicitors on 2380 patents!

The penalties inflicted on the inventive genius of Britain during the *present* year, up to the 25th ult., in the shape of government stamps and fees on patents, amount to more than £11,000!

N.B. This sum has been paid in *ready money* on taking the first steps, and as many of the inventors are poor men, (*Operatives,*) and a great many others of them persons to whom it would be very inconvenient to pay at least £100 down, they have been obliged to go into debt, or mortgage or dispose of their inventions, either wholly or in part, &c.

Razor-edge invisible in the most powerful Microscope.

"There is a very ordinary object, which I can never see with any instrument, which certainly we should expect to be visible by mere magnifying power, with the common microscope;—I allude to the edge of a very sharp razor, presented to the axis in a line bisecting the thickness of the back, so that the thickness of the said edge, or its apex, should be truly shown and measured, as it may be in a dull case-knife. I suspect the edge of a very sharp razor is the nearest approach to a *mathematical line* with which we are acquainted; it seems to resist magnifying power altogether, or at least such as we can apply to it."—Goring. *Micrographia,* 1837.

NEW PATENTS. 1837.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second that on or before which the Specification must be delivered and enrolled.——The abbreviation *For. Comm.*, signifies that the invention, &c., is "a communication from a foreigner residing abroad."

February *cont.*

43. John Robinson, North Shields, *Northumb.*, Engineer; for a nipping lever for causing the rotation of wheels, shafts, or cylinders, under certain circumstances. Feb. 28.—Aug. 28.

Total, February...19.

March.

44. David Stevenson, Bath-place, New-road, *Middx.*, Gent.; for a new method of preparing writing-paper, from which writing-ink cannot be expunged or abstracted without detection. March 2.—Sept. 2. *For. Comm.*

45. Thomas Bradshaw Whitfield, New-st.-sq., *Middx.*, Lamp-Manufacturer; for improvements in producing parallel motion to the piston-rods of pumps for lamps, and other purposes; which improvements are also applicable to machinery in general, where parallel motion is required. Mar. 4.—Sept. 4.

46. Samuel Stocker, *Bristol*, Gent.; for improvements in pumps. Mar. 4.—Sept. 4.

47. Charles François Edward Aulas, Cockspur-st., *Middx.*, Gent.; for an improvement, or improvements, in preparing writing-paper so as to prevent the discharge of the ink therefrom without detection, and to prevent the falsification of writing thereon. Mar. 6.—Sept. 6.

48. Henry Backhouse, Walmsley, Bury, Calico-Printer, and Jeremiah Grime, of the same place, *Lanc.*, Engraver; for certain improvements in the art of block-printing. Mar. 7.—Sept. 7.

49. John Shaw, Rishworth, Halifax, *York.*, Book-keeper; for improved machinery in preparing wool, and also in preparing the waste of cotton-wool for spinning. Mar. 7.—Sept. 7.

50. John Consett, Manchester, *Lanc.*, Mechanist; for improvements in the machinery used for spinning, doubling, and twisting cotton, and other fibrous substances. Mar 8.—Sept. 8.

51. Charles William Celarier, St. Paul's-chain, *Lond.*, Esq., for improvements on lamps, particularly for causing the oil to ascend, which improvements, or parts thereof, are applicable to the raising of water and other liquids. Mar. 10.—Sept. 10. *For. Comm.*

52. Neil Snodgrass, Glasgow, *Lanark*, Engineer; for improvements in steam-engines, and other mechanism of steam-boats. Mar. 15.—Sept. 15.

53. Henry Christopher Windle, Walsall, *Staff.*, Merchant; Joseph Gillott, Birmingham, *Warw.*, Metallic Pen Manufacturer; and Stephen Morris, Birmingham aforesaid, Artisan; for improved means of giving elasticity, freedom of action, and durability, to certain parts of pens or instruments used in writing; as also of obtaining a supply and flow of ink to the same. Mar. 15.—May 15.

54. Charles François Edward Aulas, Cockspur-st., *Middx.*, Gent.; for a new and improved method of cutting and working wood by machinery. Mar. 15.—Sept. 15. *For. Comm.*

55. Richard Macnamara, Hunter-st., *Southwk.*, Gent.; for improvements in paving, pitching, or covering streets, roads, and other ways, which improvements are applicable to other purposes. Mar. 15.—Sept. 15.

56. Henry Davies, Stoke Prior, *Worc.*, Engineer; for improved apparatus or machinery for obtaining mechanical power; also certain improved apparatus or machinery for impelling or raising fluids. Mar. 15.—Sept. 15.

57. William Maugham, Newport-st., Lambeth, *Surry*, Chemist; for improvements in the manufacture of white lead. Mar. 15.—Sept. 15.

58. James Walton, of Sowerby-bridge Mills, Warley, Halifax, *York.*, Woollen Manufacturer and Frizer; for improvements in machinery for manufacturing and finishing of woollen and some other cloths. Mar. 21.—Sept. 21.

59. Moses Poole, Lincoln's-Inn, Gent.; for improvements in making fermented liquors. Mar. 21.—Sept. 21. *For. Comm.*

60. Robert Neilson, Liverpool, *Lanc.*, Gent.; for a machine for preparing and cleaning coffee from the pod or husk, and separating the different qualities, so as to render it better adapted for the purposes of roasting and consumption. Mar. 21.—May 21.

61. Miles Berry, Chancery-lane, *Middx.*, Mechanical Draftsman; for improvements in machinery for heckling or combing, and preparing and roving, hemp, flax, tow, and other vegetable fibrous materials. Mar. 27.—Sept. 27. *For. Comm.*

METEOROLOGICAL JOURNAL FOR FEBRUARY, 1837; KEPT AT BLACKHEATH ROAD.

Day of Month		Barom. 9 A.M.	Ther. attch.	Barom. 3 P.M.	Ther. attch.	Thermometer Min.	Thermometer Max.	Daily Temp.	Solar Var.	Rad.	Clouds. A.M.	Clouds. P.M.	Winds A.M.	Winds P.M.	Direction of Wind A.M.	Direction of Wind P.M.	Lunation.	WEATHER, &c.
Wed.	1	30·100	50°	30·142	52°	35°4	44°9	40°2	9°5	32°	10	10	0	0	S	N		Thick misty weather.
Thurs.	2	30·382	51	30·380	53	39·7	45·8	42·7	6·1	39	10	9	0	1	SE	E		Clouds broken; fine Day.
Friday,	3	30·408	53	30·403	52	38·3	46·0	42·1	7·7	36	10	8	0	0	ESE	ESE		Ditto fine clear Night.
Satur.	4	30·460	49	30·430	49	28·6	38·5	33·6	9·9	27	4	4	1	1	SE	SE		Hoar frost; fair; *cirro-stratus* and *scud*.
SUN.	5	30·452	48	30·405	48	32·5	38·0	35·3	5·5	31	10	9	1	1	SSE	SE	●	Cloudy; Evening quite clear.
Mon.	6	30·455	47	30·414	49	26·2	37·4	31·8	11·2	25	0	0	1	1	ESE	E.		Perfectly cloudless.
Tues.	7	30·390	46	30·352	47	26·3	40·2	33·3	13·9	25	5	4	1	1	S	S.		Thin clouds, and fine.
Wed.	8	30·251	44	30·248	48	29·2	44·0	36·6	14·8	27½	6	10	2	2	S	sbw		*Cirro-stratus* and *scud*; air damp; rain P.M.
Thurs.	9	30·352	50	30·324	52	41·0	48·5	44·8	7·5	40	10	10	2	1	SSW	SW		Damp and cloudy.
Friday,	10	30·026	52	29·900	54	43·5	49·0	46·3	5·5	41	9	10	3	3	sbw	sbw		Windy and rainy.
Satur.	11	29·496	54	29·208	55	43·6	50·5	47·0	6·9	42	10	10	4·5	4·3	S	sbw		Stormy, with rain.
SUN.	12	29·562	52	29·610	53	35·0	44·6	39·8	9·6	34	3	4	2	2	SW	WSW	☽	Fine; *cumuli*.
Mon.	13	29·366	54	29·319	55	38·1	50·0	44·0	11·9	36	9	9	3	2	WSW	SW		Rain and wind; *cirro-stratus* and *scud*; fine Evening.
Tues.	14	29·440	53	29·576	54	34·4	50·5	42·5	16·1	32	2	7	1	1	SSW	W.		Dense *cumuli* into *nimbi*; large hail at 1 P.M.
Wed.	15	30·085	51	30·150	52	29·0	47·0	38·0	18·0	27½	3	3	2	2	SW	SW		Fine; *cirro-cumulus*.
Thurs.	16	30·206	53	30·203	55	43·1	54·5	48·8	11·4	41	8	8	2	2	WSW	WSW		*Cirro-cumulus*; mild.
Friday,	17	30·350	53	30·400	56	41·0	50·4	45·7	9·4	39	1	1	2	1	W	W		Very fine clear day.
Satur.	18	30·122	53	29·883	55	33·9	50·2	42·0	16·3	30	10	8	3	3	S	SSW		Windy, with light showers; stormy from 7 to 9 P.M.
SUN.	19	29·800	52	29·466	52	33·8	50·6	42·2	16·8	31	10	10	2	4	S	SSW	○	Stormy, high wind, with showers.
Mon.	20	29·596	52	29·790	53	38·2	46·1	42·2	7·9	35	2	3	3	3	WNW	wbN		Fine; *cumuli*.
Tues.	21	29·630	54	29·675	55	41·0	50·4	45·7	9·4	39	8	7	4	3	wbs	W		Squalls, with rain.
Wed.	22	29·949	52	30·022	53	37·2	47·1	42·1	9·9	34	2	3	2·3	2	W	W		Windy; fine Day and Evening.
Thurs.	23	29·725	52	29·300	54	36·6	50·0	43·3	13·4	34	10	10	3	3·4	SSW	NW		A heavy gale from N.W. P.M.
Friday,	24	29·808	50	29·946	50	33·0	42·8	37·9	9·8	32	5	5	3	3	NW	NNW		Strong wind; at 6 P.M. violent squalls, with sleet.
Satur.	25	30·182	47	30·226	47	31·1	39·2	35·2	8·1	29	3	7	2	2	N	N		Fair and cold.
SUN.	26	30·296	44	30·258	47	29·0	39·0	34·0	10·0	28	1	1	1	2	Nbw	N		Ground frozen hard. Fine. Eve cloudy.
Mon.	27	30·107	47	30·045	47	31·0	40·1	35·6	9·1	30	10	10	0	0	SW	N	☾	Cloudy and calm; *very dark day*.
Tues.	28	30·152	47	30·176	47	33·2	41·2	37·2	8·0	32	8	8	1	1	NNE	NNE		Cold drying wind. Clear at Night, with frost.
Mean		30·040	52	30·008	52	35·10	45·57	40·36	10·48									Barom. Mean 30·024 in.

Bar. Max. 30·460 in. on the 4th. | Ther. Max. 54·5° on the 16th. | Lowest point of Rad. 25°, on the 6th.
Bar. Min. 29·178 in. „ 11th. | Ther. Min. 26·2° „ 6th. | Rain fallen, 2·200 in.

ON SOME PHENOMENA CONNECTED WITH IMPERFECT VISION.

We are accustomed from education and habit, and from the manifest delight which we feel in the contemplation of the beautiful and the perfect, to view the creations of nature as exquisite adaptations to the wants of living beings *only* when their functions or properties are exerted in a healthy, sound, and fitting manner; and we are slow to acknowledge the beauty of physical laws, the operation of which interferes with our own comforts and inclination.

The sublime truths which astronomy unfolds to us, for instance, imperatively call upon us to acknowledge the exquisite Judgment and all-pervading Power which could set in motion, and preserve in systematic arrangement, the giant orbs, whose number baffles our power of conception. Yet, were any unforeseen catastrophe to disturb what we may truly term the "harmony of the spheres,"—were one of those erratic visitors, comets, for example, respecting whose origin, formation, and purpose in the starry system, we know so little,—were one of these to come in collision with our own satellite, the moon,—how fearfully would our view of the beauty and fitness of the physical laws of the heavens be shaken! their *sublimity*, indeed, our shrinking minds would irresistibly acknowledge; but the mere circumstance of losing the value of our moon's light (so much do we measure the value of nature's works by their subserviency to our interests and inclinations,) would make us doubt the consistency of the laws by which the starry firmament is governed. We are too apt to reason in this way, and the more ignorance prevails, the more does the prevalence of this selfish and ignoble line of argument show itself: the mind, incapable of grasping an ideal image of the universe as a whole, narrows its grasp to that small portion of it which our senses enable us to perceive, and estimates the beauty of that portion nearly in proportion to its uses to ourselves. Yet calm reflection should teach us to view even such a catastrophe as that above mentioned as one component element in the grand scheme of nature, by which the great Author of all conducts, combines, and arranges the whole.

So it is with the more common events which present themselves to our every-day observation; when we see either mental or bodily functions imperfectly conducted, the beauty of nature's laws is for the time forgotten, and we would fain persuade ourselves that they are suspended in that instance. But it would be both more philosophical, and more expressive of our homage to the Infinite Wisdom, if we were to detach ourselves from the mere selfish consideration of the event, and view it in connexion with the general code of nature's laws, which, the more they are examined, the more will they be found in harmony with each other.

These remarks have been suggested by a consideration of the phenomena connected with imperfect vision;—a defect which, viewed individually, we are apt to consider as an unfortunate exception to the unity of purpose manifested in the general laws relating to vision: yet, unfortunate as it certainly is *individually*, we have no right to consider it at variance with physical laws generally; and we shall better indicate our admiration of those laws, by endeavouring to trace defects in our organs of sense to some adequate cause, than by lamenting their existence as exceptions to a general rule.

The writer of this paper,—himself labouring under a defective sense of vision,—thinks it might not be an ill-disposed employment of time, either to himself or the reader, to enter upon a description of the phenomena which present themselves to him, and to endeavour to trace them to known optical laws. Facts are at all times better than theory: and he thinks that a description of these defects coming from him who has to contend against them, would possess, perhaps, more value than would attach to the explanation or supposition of one who knew not their force nor their inconvenience.

It may be as well, perhaps, to present to our readers a short view of vision generally, considered in its healthy and proper state, as a convenient introduction to the consideration of some of the causes which lead to imperfect vision;—we say *some* of them, for it is manifestly a subject for a large medical treatise to inquire into *all* the diseases of the eye.

The nature of light, its origin, composition, and laws of propagation, form an immense subject, into which we cannot now enter: we will presume our readers to be sufficiently acquainted with it to follow the brief details into which we shall enter.

The eye consists mainly of one solid and two fluid humours, and four membranous coats. The *sclerotic* coat is the exterior covering, and to it are attached the muscles which regulate the motion of the eye, and constitute that which we are in the habit of calling the *white* of the eye. This coat does not completely surround the eye-ball, as its opacity would prevent light from entering the pupil; but in its place, a circular portion in front is occupied by the *cornea*, a beautifully-transparent shield, through which the rays pass uninterruptedly. The sclerotic coat is lined internally with a more delicate membrane, the *choroid* coat, which is covered with an opaque black pigment, for absorbing the rays when the purpose of vision has been served. Within this, again, is placed the fourth coat, the *retina*, formed by minute ramifications of the optic nerve, which latter enters the eye nearly opposite to the pupil.

So much for the coating or shell of the eye. The enclosed globular space is thus occupied: a flat membrane, called the *iris*, is drawn across the globe of the eye a short distance behind the cornea, and thus divides the globular space into two unequal chambers; the anterior of which is filled with the *aqueous humour*, a very limpid fluid; immediately behind the iris is held, by a beautiful muscular apparatus, the *crystalline lens*, a double convex structure, formed of laminæ, or plates, one within another; the rest of the eye, comprising that part between the lens and the retina, is filled with the vitreous humour. The *pupil* is a perforation in the centre of the iris, for the sake of admitting the rays of light, and is reduced or enlarged by the contractile power of the iris, according to the intensity of the light to which it is subject.

This, then, being the position of the apparatus for vision, a pencil of light, emanating from any luminous object, and impinging on the cornea, is refracted by the aqueous humour, the innumerable rays of which the pencil consists being thereby brought nearer to paralellism: such of them as the iris permits to pass through the pupil are then further refracted by the crystalline lens, and the rays are by this time converted from diverging to converging,—but still not sufficiently so, and the task is finished by the vitreous humour, which converges the rays to a single point, exactly when they reach the retina; the same effect being produced on every pencil of light which emanates from the object, an exact image of the latter is represented on the retina.

Now this is the principal element in distinct vision,—the convergence of the rays of any given pencil to a point at the retina; and the non-attainment of that desirable object is an inevitable source of imperfect vision; because, if the rays do *not* meet in a point exactly at the retina, they must form a small disc thereon, and if every point of the object is represented by a disc instead of a point, on the retina, the images which ought to be separated are confused by the overlapping of the different discs on each other. This is the result either of an excess or a deficiency of refracting power in the humours of the eye. It requires a most exquisite adjustment of parts to attain the precise degree of convergence necessary; and any deviation from that adjustment produces those imperfect powers of vision termed off-sight and near-sight. The writer is subject to the latter inconvenience, and will now proceed to detail a few instances of the manner in which the proper images of objects are influenced by it.

The eye, it should here be observed, has the power of voluntary adjustment to a very great range of distance. The manner of doing this is a source of much discussion; but the prevailing opinion seems to be, that it is mainly brought about by changing the position of the crystalline lens, drawing it forward when near objects are to be viewed, and thrusting it backward when the object is more remote. A healthy eye can be adjusted to all distances, from six inches to many miles; but within five or six inches clear vision is not obtained, except in the case of near-sight. Now, it will be convenient to term the above-named range of distinct vision the *focal range;* and it will be found that this focal range is narrowed in proportion as the eye is near-sighted: in the writer's instance it is reduced to eight inches,—two inches being the minimum, and ten inches the maximum, distance at which he can obtain clear and distinct images of any object.

When he views an object at a *less* distance than the focal range of his eye, the image on the retina is confused, and is bordered with a blueish-green fringe on each edge. When, on the other hand, he views it from *beyond* the focal range, the image is bordered with a red, or rather an orange fringe. These colours are quite independent of the colour of the object itself, seeming to depend solely upon the proximity of the object to the eye. These fringes are probably occasioned by the different refrangibility of the different coloured rays of light; the red, being the least refrangible, are those which are most likely to fall on the exterior of the luminous circle which each point of the object forms on the retina, and as every *proper* image is a collection of such points, so is every near-sighted image a collection of such small circles, and the object appears bounded with an orange border, in the same way as each point would separately. This occurs when the rays are focalized *behind* the retina; but, in the other case, when viewed from beyond the focal range, they are focalized *before* they reach the retina, and the blueish-green rays become separated, and form the fringe by the time they, by diverging again, reach the retina. He has not met with any description of these fringes occurring to the near-sighted eye; but he conceives the different refrangibilities of the different colours are sufficient to account for their production.

Some remarkable instances of changes of colour, and changes from obscure to clear vision, are presented to him on viewing a striated surface at different distances. He took a piece of paper about twelve inches square, and painted it alternately in white and black parallel stripes, each about three-quarters of an inch wide. On viewing it within his focal range (from two to ten inches), the lines are clearly marked and perfectly distinct; but as the distance was gradually increased, some singular changes occurred. The outlines become gradually confused, the black stripes go through all the gradations of black, purple, dark blue, light blue, and blueish-white; and the white through the gradations of white, yellow, orange, dusky red, and brown; and when the black and white have changed respectively to blueish-white and brown, the outlines again become clear and distinct;—this is at the distance of three feet six inches. Still receding, the outlines become again confused and indistinct: the blueish-white stripes gradually return, in a reverse order, to a brown or dusky red, and the brown returns to a blueish or impure white, and a third time the outlines become again clear and distinct, at about five feet six inches. Again increasing the distance, the first order of changes is again repeated, and at about seven feet the outlines are again clearly defined; and even after this, a *fourth* order of changes occur and terminate at eight feet distance.

These remarkable effects are the result of superposition of neighbouring fringes on the retina. At the focal range of the observer's eye, the dividing lines between the black and white are accurately depicted on the retina; but beyond that distance, the black encroaches on the white, and the white on the black, and it is at such a time that we become cognizant of the compound nature of white light. The red and blue rays being differently affected by refraction, are separated in some degree from each other; and the blue rays of the white stripes overlapping the black, gradually produce the changes in the latter which have been mentioned, while the less refrangible rays (the red, orange, &c.) being thus in some degree isolated, present only their own colour from those stripes which were originally white. This separation begins directly the eye is removed beyond the focal range, and goes on gradually until the fringed borders of each white stripe are exactly equal in width to the adjoining black stripe, at which period the adjoining overlappings exactly coincide, and the outlines become distinctly marked. Now, bearing in mind that at the end of what we will term the first *cycle of changes*, the image of each white stripe is three times its proper width, we shall easily understand that when it becomes five times its proper width, (and the width increases with the distance,) the overlappings will again exactly coincide, and the outlines again become distinct, but the resulting colours cannot be exactly the same as at the second cycle, because the overlappings extend to two striæ on each side of each white stripe, instead of one.

Pursuing the same train of reasoning, we arrive at the conclusion that when, by increased distance, the image of each white stripe becomes seven times its proper width, the outlines will again become perceptible by the coincidence of adjoining fringes: these periods occur, as above stated, at (in round numbers) 3½, 5½, 7, and 8 feet distance, beyond which they recur rapidly, but with less and less distinctness.

But these cycles of changes he has observed in a beautiful manner, and at one view, by taking a circular disc painted with black and white radii, as in fig. 1. When held near the eye, the lines of division appear correctly preserved; but on receding from it, the parts of radii near the nucleus at *a* begin to change and overlap each other, and at a very few inches a cycle of changes takes place at the point; at a greater distance the central portions of the radii begin to overlap while the first portion is performing its second cycle; on gradually increasing the distance, the wider portions of the radii become, bit by bit, involved in the same changes, and at about three feet distance (supposing the greatest width of the radii to be about ¾ of an inch) the fan presents a singular and beautiful appearance; there are at least half-a-dozen alternations of colour in each stripe, separated by cloudy and indistinct belts. Fig. 2 will give some idea of this: the colours at the distinct parts are, generally speaking, a faint blue and a reddish brown, but less definite at the narrow than at the wide parts, on account of the greater number of superpositions which constitute them.

Fig. 1.

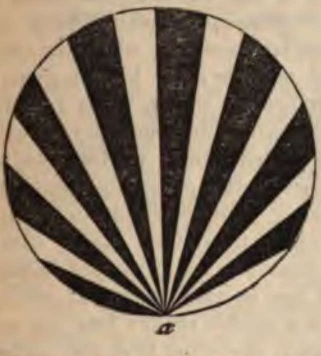
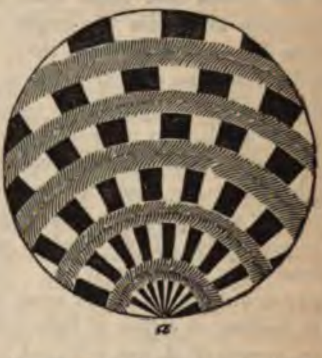


Fig. 2.



Thus this remarkable effect is produced by the simple circumstance which militates against distinct vision,—viz., the rays coming to a focus before they reach the retina, and, after focalizing, diverging again, and forming on the retina a circle or a belt when there ought only to be a point or a line: these singular appearances, as they are the growth of near-sight, have probably not been noticed by those whose eyes are well adapted for distinct vision; but it is by no means improbable that they would become evident to most persons by using *convex lenses*, such as the spectacles which are generally required when age, by altering the refracting power of the eye, renders it *far-sighted:* these glasses, used by a correct healthy eye, produce a combination nearly equivalent to the near-sighted eye *without* glasses; and it would be interesting to trace how far the same result would obtain under such circumstances.

There is a phenomenon which the writer has frequently observed with much attention, and which he has heard described by others, who, like himself, are near-sighted, and which, from opinions which have been expressed upon it, deserves consideration: it is this:—on looking at a luminous body, such as a gas or candle-flame, at several yards' distance, the luminous image (circular at that distance) is speckled with black spots continually floating about in the disc of yellow light; these spots vary incessantly in number, size, and position; are generally black, but sometimes for an instant coloured: there are likewise floating lines across the common disc, generally black, but, like the spots, occasionally coloured. There is in the pathology of the eye a disease termed amaurosis, one of the symptoms of which is, black spots in the field of vision; but in the instance now before us, the black spots are *never seen* but on viewing a flame or bright light, and *never on any occasion* when concave spectacles used; it is only when the rays are focalized anterior to the retina that these spots appear. It is not, however, the writer's intention to discuss the origin or nature of these spots, but to speak of those few occasions when they, or the lines, are *coloured:* whenever that is the case, one side or edge is always *red*, and the other blueish green; thus adding another to the number of instances in which, when colours appear at all, they consist of two which are nearly complementary to each other (that is, two, the combination of which would produce white light); thus, as described in the former part of this paper, the result of viewing an object at a less or greater distance than the focal range, was the production of two borders or fringes, which, though not exactly complementary, were nearly so. The striped papers, again, presented results approaching towards the same point; and here is another similar effect. On looking at a line of light reflected from a double convex lens, we see *two* images of it, one reflected from the upper, the other from the under surface; now the reflection from the under surface is the same in character as from a *convex mirror*, and therefore has a different focus from that of the upper surface, and the eye consequently cannot at any distance receive both reflections at once at a proper focus. Now, when the writer holds the lens at a distance which is too great for one focus and too small for the other, he sees one image bordered by a slight orange-red fringe, and the other by a slight blueish-green fringe: upon substituting a double convex for a double concave lens, that image which before is orange becomes greenish-blue, and the converse.

The last instance which he will mention, is produced when he views two flames which are so near each other that their images respectively overlap on the retina: the overlapping portion has invariably a *red*, mottled appearance, with a greenish-blue border, which separates it from the other parts of the luminous image.

The theories which account for the appearance of complementary colours are too extensive to be alluded to here, the writer's object being *to detail facts as* they occur to himself; but there is much probability that the different refrangibility of different rays, acting upon a retinal image which is not at the same time a *focal* image, is the principal cause of the chromatic changes which this paper is intended to describe.

In conclusion, it may be stated that this paper is neither intended as a treatise on vision, nor an inquiry into the diseases of the eye, but as a statement of facts which do not in general come under the notice of those who, happily for themselves, have perfect vision, and as an endeavour to show that such facts, when properly understood, are not only not contradictory to general optical laws, but are a beautiful verification of them.

If those who have to contend against defects in the organs of sense were to endeavour to analyse and describe the causes of them, according to the evidence of their own feelings, science would be a gainer by it: with such a hope has this paper been written.

ON DIETETIC CHEMISTRY.

III.

ASTRINGENT AND AROMATIC PRINCIPLES IN VEGETABLES.—TEA AND COFFEE.

HAVING, in the two preceding papers, given some account of the simple substances, or ultimate elements, of which our food consists, and of their relative proportions in the immediate or proximate principles of vegetable nutrition, we may now advert to some other articles, of less actual importance, but which, nevertheless, constitute legitimate objects of inquiry with the dietetic chemist,—such, for instance, as *astringent and aromatic substances;* these have reference to the wholesomeness of our diet, and stamp upon it, in many cases, very important peculiarities.

The meaning of the term *astringency* is well understood; it is a quality more or less agreeable as it is more or less intense, or more or less tempered by, and associated with, sweetness, with acidity, or with aroma. There are many fruits on which a slight degree of astringency confers, in conjunction with sweetness, a peculiarly pleasant flavour, such as some pears and apples, certain grapes, strawberries, and cherries, the cranberry, pomegranate, and others; but with reference to articles in daily use, TEA especially attracts the chemist's notice, as a powerful, pleasant, and salutary astringent.

Medicinally speaking, astringents rank among *tonics;* they increase the *tone*, as it is called, of the system, corrugate the fibre, and, in moderation, help the energies of a languid stomach. They are *chemically* distinguished by two very striking and infallible characters—first, they form insoluble combinations with animal jelly; and consequently, when any liquid holding vegetable astringent matter in solution is added to calf's-foot jelly, or, what amounts to the same thing, to a solution of isinglass, the mixture becomes more or less turbid, according to the proportion of astringent matter which is present; and if it be there in any quantity, a white insoluble compound falls, which subsides to the bottom of the vessel, and may be collected upon a filter, and which, when carefully dried, is composed of about forty parts of astringent matter, and sixty of jelly: hence the use of this precipitate, both in qualitative, and quantitative analysis.

Now, the *principle* which gives this astringency is the same as that which confers upon oak-bark its property of converting skin into leather; skin is, in fact, merely organized jelly, or *gelatin;* and the above-mentioned precipitate is a form of leather; hence the terms *tannin*, or *tannic acid*, applied to this astringent principle.

We may, perhaps, pause for a moment here, to remark upon the extraordinary change in the qualities and general properties of substances which is induced by chemical action or combination; in the present case a soluble, changeable, and sapid substance (namely, the astringent principle), uniting with an animal matter, which, in its humid state, is of all others most prone to putrefaction and decay (namely, gelatin), forms that most imperishable of all organic combinations—leather. An Egyptian mummy is irrelevant from our present purpose, otherwise we might adduce those relics, in addition to the evidence of the gravedigger in Hamlet, as a proof of the permanence of this curious compound; for there can be little doubt that the ancient Egyptians superadded *tanning* to their other preservative processes.

Secondly, There is another property of vegetable astringents which is also a highly characteristic test of the presence of *tannin*, namely, its blackening power over the salts of iron. This, as well as the former, is a most important property in reference to the arts; for the *blacks* of the dyer and calico-printer are exclusively derived from this source, and *writing-ink* is merely this compound suspended in gum-water. The well-known blackening of certain fruits and vegetables, when they are cut with a steel knife, is referable either to the presence of tannin, or to a mixture of tannin and *gallic acid* (so called from its existence in the gall-nut), a substance into which tannin is easily convertible, and which, therefore, is frequently associated with it.

Connected with the astringent principle in vegetables, there is often a quantity of soluble matter, to which the term *extract* is applied; what place should be assigned to it as a *nutritive principle*, is, perhaps, difficult to determine, nor is it easy to define its chemical characters, for it is almost always associated with gum, sugar, starch, or other matters soluble in water, so that we are often in the habit of calling anything that is obtained by boiling a vegetable in water, and then evaporating the decoction, an *extract;* it is thus that we speak of *extract of malt*, *extract of liquorice*, &c. The *bitterness* of some vegetables is supposed to arise from a modification of extractive; and when this is very slight, and conjoined with mucilaginous sweetness, or with aromatic pungency, it is not altogether an unpalatable combination: the bitter flavour of candied orange and lemon peel, and of angelica root, are instances.

Extract is also often the seat of *colour* in vegetables, and the coloured juices of fruits, flowers, and other parts of plants, not unfrequently come under this denomination. Among its chemical characters, that which is most distinctive is, its property of combining with aluminous earth, so that if a solution containing extract be mixed with a solution of alum, and precipitated by an alkali, the alumina and the extract fall in such intimate and permanent combination, as to leave the water which held them dissolved, colourless. Upon this simple fact depends nearly the whole art of dyeing and calico-printing; and it furnishes a useful test of the presence of certain forms of extract in dietetic products.

The *aromatic* or *odorous principle* of vegetables is another of their proximate component principles claiming the attention of the dietetic chemist, not, perhaps, essentially *nutritive*, but contributing by its stimulating nature, or by its pleasant odour, to the wholesomeness on the one hand, and to the agreeableness on the other, of many articles of food. How singularly the taste of bodies is influenced by their odours, and how closely these two properties are mutually connected, is easily proved, by holding the nose so as to close the nostrils, while partaking of some fragrant delicacy: if we thus chew a piece of cinnamon, it has no more flavour than a deal shaving; a glass of Burgundy degenerates into tavern-port; and the exquisite flavours of green tea and well-made coffee are lost in vapid bitterness.

These characters of odour and fragrancy are, in the greater number of cases, dependent upon a distinct volatile principle, which can be separated by distillation, and which has, therefore, been termed *volatile* or *essential oil*. These oils are infinitely various, as is the infinity of odours; yet their analysis points them out as *hydrocarbons;* that is, binary compounds of carbon and hydrogen, or as *oxides of hydrocarbons*. The essential oils of orange and of lemon-peel, and oil of turpentine, when pure, are of the former class; and the oils of cinnamon, cloves, lavender, roses, &c., of the latter.

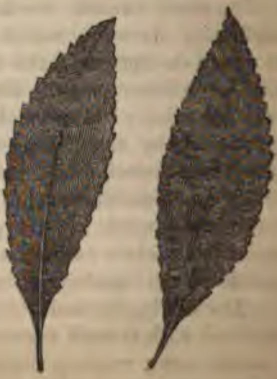
But the odorous parts of vegetables are not in all cases thus tangible: they sometimes are of so delicate and refined a nature as to elude our grasp altogether; and to form a kind of connecting link between the ponderable and confinable, and the imponderable and unconfinable forms of matter. The odour of *musk* (which, however, is an animal product,) furnishes a case in point: a single grain will continue to scent the air of a large room for years, and yet not lose perceptibly in its weight. They are also sometimes so easily decomposed and unstably constituted, as to be destroyed by the application of heat, and modified by the most careful methods of separation—hence, a number of undefinable flavours, which we cannot trace to their source or cause, but which do not elude the delicacy of our olfactory or gustatory organs: such, for instance, are the different flavours which characterize different kinds of wine; often unheeded by the unobserving, but properly appreciated by the connoisseur, under the term *aroma*, or the more appropriate French word *bouquet*. Analogous, and equally refined, but equally undefinable flavours, belong to some of our fruits: the varieties of the strawberry, of the pine, the melon, and of pears and apples, suggest themselves as instances. In many coarser vegetables, too, much depends on this quality, as in balm, parsley, celery, &c.

The astringent and aromatic principles which have been alluded to, combined with certain *vegetable acids*, with modifications of sugar, and, in some cases, with peculiar acrid oils or principles, as in pepper, mustard, horse-radish, &c., and aided, in many cases, by salt, give rise to a series of natural and artificial combinations, which, though not, strictly speaking, nutritive, lend their aid, in the form of sauces and condiments, to the digestive powers.

Upon these curious subjects, details would occupy more space than we can here command; but we may illustrate them generally, by a few words on the chemistry of tea and coffee,—articles of no mean importance, when we recollect that the annual exports of the former from China amount to about 50,000,000 lbs., of which about 32,000,000 lbs. are for home consumption.

Tea is well known as the leaf of a hardy evergreen shrub, from three to six feet high, a little resembling the broad-leaved myrtle: it is polyandrous, and of the natural order *Columniferæ;* its blossoms white, with yellow style and anthers, much like the common dog-rose; the branches are numerous and full of leaves, and the leaves are long, serrated, rather pointed, fleshy and smooth, like those of some species of *camellia**. It will grow in our green-houses; and in warmer and more steady climates has been cultivated in the open air, especially in South America and in Australia; but for all purposes of commerce, the growth of good tea is confined to certain provinces of China. There are several denominations of tea, but, without entering minutely into these, we may consider them under the general heads of *black* and *green*. According to some, these are two distinct species; but to others, mere varieties of one species, like those of the vine, climate, soil, aspect, time of gathering, and method of drying and managing the crop, being the cause of the difference. In fact, chemically speaking, black and green tea closely approximate; the black containing, perhaps, more extractive and less tan† than the green; fine green tea is also distinguished by its refreshing and agreeable odour or perfume, evolved when acted on by hot water.

The proximate principles which the chemist finds in tea are, tannin, extract, resin, essential oil or aroma, and lignin or woody fibre: but the *extract* includes a peculiar bitter principle, probably belonging to that extraordinary class of vegetable products which have been termed *alkaloids*, and of which *morphia* from opium, and *quinia* from yellow



* The annexed is a representation of a tea-leaf: it is placed here as a useful reference, for the purpose of distinguishing the genuine plant from leaves with which tea is occasionally mixed or adulterated.

† Green tea contains, on the average, a larger proportion of soluble matter than black tea; the leaves of the former, when deprived of all soluble matters by the continued action of boiling water, and afterwards of boiling alcohol, leave an (average) insoluble woody residue amounting to 57 *per cent.:* the residue of black tea amounts to 63 *per cent.* When all the soluble matter of either tea has been abstracted by water, the remaining leaves, dried, and digested in alcohol, yield from 8 to 10 *per* cent. of a fragrant and highly-flavoured *resinous extract.*

Peruvian bark, furnish such interesting examples. Such a substance has not, perhaps, been hitherto satisfactorily ascertained to be the stimulating and exhilarating principle of tea, though many circumstances tend to show its presence, and among them, the white precipitate, which a strong infusion of tea yields, with tincture of galls, or gallic acid. The substance described by Oudry under the name of *thein* is probably this principle.—(*Brande's Chemistry*, p. 1048.)

It is not improbable, that much of the difference between black and green tea may arise from the greater or less heat to which the leaves have been exposed during the *manufacture* (as it is called) of the tea, as carried on by the Chinese; for the leaves are dried in rooms heated by charcoal fires; and in the process of *making*, that is, of rolling and twisting them up, they are submitted to a high temperature in shallow iron pans. Much, therefore, of the flavour and quality, or, in other words, of the composition of tea, must necessarily depend upon the heat applied, and upon the number of dryings; but the real extent and nature of such changes can only be learned from a careful analysis of the leaves before and after manufacture; that is, in their fresh, and in their prepared state: for the flavour and characters of the green leaf are very distinct from those of the dried and prepared tea.

It may not be irrelevant to add here a few remarks on the varieties of tea with which we are most familiar. Of black teas, *Bohea*, or, more properly, *Voyce*, is the name of a *district; Congou* means *care in making; Sou-chong* is *little and good;* and *Peko* signifies *white leaf.*

With us, *bohea* is the name of the commonest *black tea;* it is distinguished in the trade as Canton bohea, or *worst*, and Fokien bohea, or *best*. *Congou*, of which there are several kinds, occupies a place between bohea on the one side, and *souchong* on the other; but although this latter term is commonly applied, real souchong is a very scarce article, and the tea usually sold under that name is a very fine kind of congou. The kind of tea constituting the worst souchong and the best congou, is termed *campoi.*

Souchong has a fine and delicate flavour, and generally has pale leaves mixed with it: when without pale leaves, and with a certain mixture of white shoots, or of flowers (of the *olea fragrans*), it forms *flowery peko. Ancoi* (from the name of a province), is also a grade of this tea. *Caper* is fine congou or souchong, rolled up into small globular forms; and *orange peko* is a very choice, highly-flavoured tea, sometimes perfumed, and distinguished by its small and wiry leaf.

Mr. Reeves (*Evidence before a Committee of the House of Commons*) has given a very intelligible statement of the origin of these varieties. About the month of May, when the pickings begin, the tea-tree is in full leaf, and ready to throw out young shoots. The first white shoot, on the bud coming out, is covered with hairy filaments, and forms fine *flowery peko.* After a few days' further growth, the hair falls off, the leaf expands, and it becomes *black-leaf peko.* The fleshy and fine leaves of the young shoots form *souchong;* the next best leaves make *campoi;* the next, *congou;* and the refuse leaves, *bohea*. These are the distinctive terms under which the teas are purchased of the farmers by the manufacturer, by whom they are afterwards variously mixed.

The varieties of *green tea* are, *twankay*, and several kinds of *hyson*: the former is a coarse article. *Hyson* includes the finer kinds of green tea. The term *hyson-skin* is applied to the least perfectly rolled, and, therefore, lightest leaves. The finest hyson has a bright leaf, fresh flavour, and is well made or *twisted*. *Imperial* and *gunpowder* are more rolled and globular, the latter being finest and smallest. There is also *twankay-imperial*, and *twankay-gunpowder*. The *siftings*, that is, the smaller leaves, are called *young twankay* and *young hyson*.

There is scarcely any article the delicacy of the flavour of which is so easily impaired as tea; hence the necessity of great caution in packing and warehousing it. Even the paper in which it is wrapped must be scrupulously looked to. The miscellaneous collections of articles in grocers' warehouses are often unfavourable to the delicacy of tea; and the trash of chandlers' shops is fatal to it.

In our market, tea is judged of, in the first place, by its general appearance and character, and the colour and state of the leaves, as being well or ill made; and, secondly, by its touch and weight. All the best teas are heavy, and therefore the least bulky for equal weights. The smell of tea is also an important guide, and infusions are made of each sample, by the flavour, colour, and characters of which, the broker completes his judgment of this important article.

The *adulteration of tea* has sometimes been carried on to an enormous extent, and the details of the excise prosecutions, in reference to this nefarious traffic, have disclosed some curious information. Sloe, ash, and elder leaves, are the usual sources resorted to; and to a common and careless, or hasty observer, the imitations are not bad, especially when the leaves have been diligently rolled and twisted, and skilfully dyed by logwood, or a salt of iron. In regard to *green tea*, however, the matter is more serious, the colour being given by a mixture of Dutch pink and verdigris, or carbonate of copper. These frauds are detected by infusing the leaves in warm water, so as to unroll them, when their forms may be examined, and compared with those of genuine tea; by the colour, taste, and other qualities of the infusion; and by the blue colour which they communicate to liquid ammonia, in those cases where the bloom is given by copper.

Coffee is another luxury, in which tan, extract, aroma, and a peculiar bitter principle (caffein) are originally existent; but these substances are materially modified by *roasting*. It is the berry of the *Coffea Arabica*, a native of Arabia, but largely cultivated in India, Java, the West Indies, Brazil, &c. When coffee is roasted, it loses considerably in weight; to the amount, namely, of 12 *per cent.* when pale brown, and 25 *per cent.* when nearly black: the average loss is about 15 *per cent.*; of this loss, a large proportion consists of *caffein*, a peculiar volatile crystallisable and bitter principle, and which is by some regarded as the stimulating or exciting principle of coffee; but of this we have no direct evidence. According to Chenevix, tannin is produced during the slight roasting of coffee, and it is probably converted into gallic acid as the process proceeds; for coffee which has been slowly but perfectly roasted, often acquires a decided acidity. The aroma, or flavouring extractive of coffee, is *also produced by* the action of heat upon the principles of the raw grain

The following is the average composition of well-roasted mocha coffee, but the relative proportions of its proximate principles will, of course, be liable to much variation, according to the manner and extent of its torrefaction, and to the original quality of the raw coffee:—

Extractive matter, soluble both in water and in alcohol, and resembling that obtained from the unroasted berries, but browner, and more deliquescent	125
Brown gum	104
Brown extractive matter, soluble in water, but insoluble in pure alcohol	57
Oil and resin	20
Insoluble and charred woody fibre	694
	1000

The ground coffee sold at chandlers' shops is generally adulterated with parched peas and beans, or with roasted corn; and such adulterations are not easily detected, except by the inferior flavour, and entire want of aroma in the infusion or decoction: it is well, both in regard to coffee, and to pepper, and other articles, to avoid their transit through the grocer's mill. The existence of a mischievous and short-sighted propensity in the public for everything that is *cheap*, is a great and permanent incentive to *adulteration*, a branch of art in which uncommon skill and ingenuity are often displayed; but, from legislation downwards, nothing that is cheap can be good*.

[To be continued.]

ON EVAPORATION, VAPORIZATION, AND CONDENSATION.

II.

Having, on a former occasion (Vol. II. p. 125), announced some general principles relating to evaporation, vaporization, and condensation, we now propose to lay before the reader a more particular account of these interesting phenomena.

Heat is the cause of evaporation.

The *quantity* of vapour existing in any given space depends entirely upon temperature, and is in no respect influenced by the presence or absence of air or any other elastic fluid.

If we take a glass flask, perfectly dry, and so fitted up, that its interior may be made freely to communicate with a small vessel of water, the weight of which, as well as that of the flask, must be separately ascertained, we shall be able to prove, that the quantity of vapour in the flask will always be proportionate to the temperature. For example: when a thermometer placed within the flask indicates a temperature = 32°, if we detach the vessel of water and weigh it, we shall find that only a very small quantity of the water has disappeared; which having entered the flask in the form of vapour, will, of course, increase its weight precisely in the same proportion as that of the water has been diminished. The flask and vessel being again connected, and the temperature of the former raised to 40°, an additional quantity of vapour will be found to have passed into it. At 50° it will contain more, and at every successive increase of temperature the quantity of vapour will be still further increased. If the temperature of the flask be reduced, say from 70° to 40°, part of the contained vapour will be converted into water; the portion that remains being exactly equal to that which entered the flask when its temperature was originally 40°.

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* A curious illustrative case came before the writer of this article. He was visiting a factory of carbonate of soda, and observed, in a yard attached to the premises, several large stacks of calcined sawdust (in other words, of coarsely-powdered charcoal), in a damp state; it was a waste product of the process. On inquiring whether it was applicable to any purpose, he was informed that a lucrative market had lately presented itself, and that it was eagerly purchased by an eminent chimney-sweeper in the negihbourhood, *for the purpose of adulterating soot.*

The foregoing experiments we have supposed to have been conducted with a flask filled with air at the ordinary pressure of the atmosphere, that air being perfectly dry, and no part of it permitted to escape. The conditions may be varied by withdrawing the greater portion of the air from the flask, which must again be dried, and its weight carefully noted. Proceeding in the way already mentioned, it will be found that the quantity of vapour in the flask will be the same at every corresponding degree of temperature when emptied of air, as when it was full. A remarkable difference will, however, be perceptible in the *rate* of evaporation under the circumstances just described. When the flask is empty, it receives its proper complement of vapour in an instant; but in the former series of experiments, the flask being previously full of air, it was necessary that considerable time should be permitted to elapse between each observation. This was to enable the vapour to find its way among the particles of air into every part of the flask, so as fully to saturate its contents.

From these and a variety of other equally satisfactory illustrations, we infer that evaporation is not, as was formerly imagined, dependent on any chemical affinity subsisting between air and water. The *amount* of evaporation is determined solely by temperature (all other conditions being equal); but the *rate* of evaporation is impeded or accelerated just in proportion as the density of the air in contact with the evaporating liquid is increased or diminished. So far, therefore, from air exhibiting anything like an attraction for water, and thereby promoting its evaporation, it offers an impediment to the process as respects the time that would be otherwise required to fill a given space up to the point of saturation.

That vapour unites mechanically, and that it does not combine chemically, with air, may be further illustrated by a reference to some of the early labours of Dalton in this department of physical science,—a philosopher to whom we are chiefly indebted for having established what is now admitted to be the only true theory of spontaneous evaporation. The experiments to which we particularly allude may be thus briefly described. Dalton took a tube, graduated into inches and tenths, sealed at one end, and similar in its dimensions to those used for barometers.

The tube was made perfectly dry, then filled with recently-boiled mercury, and its open end immersed into a cistern containing the same metal. A portion of dry atmospheric air was then introduced into the tube, and its bulk and temperature carefully ascertained. This air of course occupied the upper part of the tube, that is, the space above the mercury, which in barometers is termed the Torricellian vacuum. The air being gradually heated, its rate of expansion was observed for each degree of temperature between 32° and 212°. Dalton next proceeded to determine the tension, that is, the elastic force, of pure aqueous vapour at every degree throughout the same range of temperature. This he accomplished, by putting a small quantity of water into the tube, instead of air, and heating it as already mentioned. Lastly, by introducing dry air again into the tube, and some water with it, the rate at which air in contact, or rather saturated, with aqueous vapour dilated, was ascertained for the various degrees of temperature before indicated.

The results of these researches of our justly distinguished countryman were highly instructive. It was found that the expansive force of dry air at any particular temperature, as exhibited in the first series of experiments, being added to that of aqueous vapour at the same temperature, as determined by the second, was exactly equal to their joint expansive force (temperature being still the same), as denoted by the third. It is obvious, therefore, either that no chemical combination takes place between air and vapour, or, if any such combination exist, it is of so feeble a character as to be wholly inappreciable, and, consequently, that it cannot in any way affect our estimate of the mechanical properties and general habits of the bodies under consideration.

This is a subject which at different periods has been very ably investigated by men*, foreigners as well as natives, who are as eminent for the originality of their views, as they are for the accuracy of their observations. By methods differing essentially in their details from those pursued by Dalton, now almost forty years since, the same results have been obtained. Perfect uniformity, in every instance, was not to be expected. Considering, however, the simplicity of the apparatus employed by Dalton, it is greater matter of surprise that the errors in his original experiments were not more numerous than that they were so few.

Dalton and others who have been engaged in these investigations, have not confined them to air and aqueous vapour; nor have they been limited to the range of temperature above mentioned. Gases in general, as well as vapours, have been scrupulously examined, at almost every degree of temperature that is practicable, and the results having been reduced to a tabular form, has imparted to them a greater value than they would have otherwise possessed.

Other circumstances being the same, it is found that *all* gaseous bodies expand equally by equal increments of heat. The rate of expansion and contraction, as exhibited by any particular gas through a certain range of temperature, being therefore accurately determined, the knowledge so obtained is applicable to all other gases. Vapours, also, so far as they have been hitherto examined, are obedient to the same law as gases with respect to heat; provided, however, that in every instance the vapour is kept apart from any sensibly-evaporating fluid, and its temperature not reduced below the limits of saturation. Whenever this happens, a portion of the vapour will be condensed, and the operation of an otherwise simple and uniform law is interfered with. It is proper, also, to notice, that some anomalous results have been observed, with respect to the relation which was supposed in every case to exist, between the tension of a vapour and the boiling-point of the liquid from which it is generated. In a great number of instances this law holds good; but it is not of universal application.

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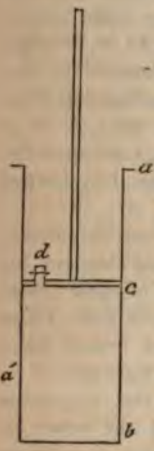
* Deluc, Desormes, Gay-Lussac, Despretz, Ure, Daniell, &c. &c.

Another fact connected with evaporation we must not omit to mention, namely, that the tension or elasticity of vapour will continue the same so long as the temperature remains constant, whatever variations of pressure may occur in the air or gas with which it is united, provided there be always present sufficient liquid to keep up a supply of vapour proper for the temperature to which the mixture may be exposed. Let *a b* represent a vessel open at the top, whose capacity is equal to a hundred cubic inches, and which has a plug, or piston, *c*, moving air-tight within it. The piston being in the situation as denoted at *c*, there will be fifty cubic inches, say, of air, in the space between *c* and *b*. A stop-cock at *d* being open, the air below the piston communicates with the atmosphere, and, consequently, is not subject to any pressure. There is, however, some water in the vessel, and hence the air will be saturated with vapour. Its temperature we will suppose to be 60°. If the stop-cock *d* be now closed, and the piston moved from *c* to *a*, the air below it will be made to occupy the whole of the vessel, in other words, fifty inches of air will suddenly become a hundred inches. At the moment when this enlargement of the volume of air takes place, the vapour mixed with it will expand, and its tension be diminished in the same proportion; the change being also accompanied by a slight reduction of temperature. This state of things will not long continue. The temperature (60°) being restored, the vapour will soon recover its former elasticity, a supply proportioned to the additional space occupied by the air being furnished by the water in the vessel. Had there been no water present, the vapour, obedient to the same law as air, would have had its elasticity diminished in the same ratio that its volume was increased.



Again, we may suppose the piston *c* to be moved from *a* to *a′* (the stop-cock being shut), when the air which before occupied the whole of the vessel will be driven into one-fourth of that space. In this instance the air will have its elasticity increased in the same proportion that its volume is diminished; its sudden compression being also accompanied by a slight increase of temperature. In a few minutes the original temperature (60°) will be regained, and then the vapour will have accommodated itself to its altered circumstances, a part of it having resumed the liquid state, that which remains mixed with the air will manifest the same tension that it did when occupying a space equal to fifty and a hundred inches, respectively.

When 100 parts (say cubic inches, for instance) of dry air, whose temperature is 32°, are heated to 212°, they expand to 137·5 parts, the increase for 180 degrees ($212^\circ - 32^\circ = 180^\circ$) being 0·375 of the whole bulk. If we divide this total increase of bulk by the total difference of temperature ($\frac{0\cdot375}{180^\circ} = \cdot002083$, or $\frac{1}{480}$), we find the rate of expansion for every degree from 32° to 212° is $\frac{1}{480}$ th part of the volume at 32°. We thus obtain the data necessary for calculating the changes which occur in aëriform bodies by variations of temperature; and let it be remembered, that they are equally applicable to vapours as to the permanent gases*, still bearing in mind the special conditions already referred to, namely, that the vapour must be kept separate from any evaporating fluid, and its temperature maintained sufficiently high to prevent any part of it from reverting to the liquid state.

With equal precision the law just announced applies to mixtures of gases and vapours, the latter of course being subject to the above exceptions; but neither the gases nor the vapours must have any chemical affinity for each other.

In making observations on aëriform bodies, the *pressure* to which they may be exposed must always be regarded. When the pressure is not the same throughout a series of experiments, an allowance must be made, as we shall notice more fully by and by, for the variations. Pressure is so important a condition in estimating the properties of gases and vapours, that when it is said they all expand equally by equal additions of heat, it is presumed that they are all subject to equal pressure. When confined in close vessels, so that they cannot enlarge their volumes, equal quantities of heat produce equal increments of tension or elasticity.

As it is desirable to know how to estimate the effects of variations of temperature and pressure in gaseous bodies, we here introduce a few examples illustrative of such cases as we think are most likely to occur in practice. First of all, however, we must explain what is meant by *mean temperature* and *mean pressure*.

To impart a determinate and uniform character to researches connected with aëriform bodies which may be conducted at different times and places, and by different individuals, it has been found convenient to fix on a certain *temperature*, to which results obtained at all others must be reduced. The standard so adopted is termed *mean temperature*, which in this country is 60° F. The process of reducing the results obtained at any other temperature to that of 60°, is denominated *correction for temperature*.

Variations in pressure are likely to occur as often as those of temperature. They may sometimes be the natural result of atmospheric changes, at others they may be inseparable from the methods employed in seeking the information required. In either case, a certain allowance must be made for every deviation from the common standard of comparison, called *mean pressure*, and which is always understood to refer to a column of mercury thirty inches in height, as indicated by a good barometer, corrected to the mean level of the sea. The process by which variations of pressure are computed is termed *correction for pressure.*

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* In the habits and general properties of gases and vapours, the analogy is so conspicuous, that there seems every reason to conclude, as we intimated in a former paper, that they consist of the same forms of matter. The usual method of classifying them is to be viewed, therefore, as arbitrary, or a matter of convenience, rather than as warranted by any real difference in their constitution. See an account of Faraday's method of condensing into liquids nine of the gases usually termed *permanent* elastic fluids.—*Phil. Trans.* 1823. p. 189. *Thomson's Heat and Electricity*, p. 236.

We now give a few examples of the methods by which corrections both for temperature and pressure are effected.

Ex. 1.—100 cubic inches of gas in a vessel standing over water or mercury, its temperature being 40°, what will be its bulk at 50°?

At page 257 it has been shown, that the rate of expansion of gaseous bodies is $\frac{1}{480}$th of their volume at 32°. Let us suppose 480 inches of air or gas at 32° to be gradually heated to a temperature of 60°. At 33° the 480 inches will become 481, at 34° 482, at 35° 483, and so on, for every degree of temperature, until at 60° they become 508 inches. Applying this principle to the case before us, if we add 1 to 480 for every degree above 32°, we have the rate of expansion at any required temperature greater than that we started from, and by deducting 1 from 480 for every degree below 32°, we obtain the rate of contraction at any temperature less than our starting point. Whatever, therefore, may be the nature of the required corrections, *the rate of expansion at* 32° must be a constant element in the process as respects temperature.

The correction demanded in Ex. 1. may be thus obtained, namely,—

Multiply the given volume (100 cubic inches) by the rate of expansion at (50°) the required temperature (50° − 32° = 18 + 480 = 498); divide the product by the rate of expansion at (40°) the observed temperature (40° − 32° = 8 + 480 = 488), and the quotient will be the volume, corrected as required, = 102·05 cubic inches.

$$(100 \times 498 = \frac{49800}{488} = 102{\cdot}05).$$

Ex. 2.—Correct 100 cubic inches of gas at 45° to mean temperature.

Proceed as already directed. The volume in this case is = 103·042 cubic inches.

$$(100 \times (60^\circ - 32^\circ = 28 + 480)\ 508 = \frac{50800}{(45^\circ - 32^\circ = 13 + 480)\ 493} = 103{\cdot}042).$$

Ex. 3.—100 inches at 70°, to be corrected to mean temperature.

Here the corrected volume will be found = 98·07 inches.

$$(100 \times (60^\circ - 32^\circ = 28 + 480)\ 508 = \frac{50800}{(70^\circ - 32^\circ = 38 + 480)\ 518} = 98\ 07).$$

Ex 4.—100 cubic inches, temperature = 34°, barometrical pressure = 29·5 inches, to be corrected to mean temperature and pressure.

Here, it must be observed, *two* corrections are required. It matters not which is first performed. We begin, therefore, with temperature, the result of which is = 105·4 inches.

$$(100 \times 508 = \frac{50800}{(34^\circ - 32^\circ = 2 + 480)\ 482} = 105{\cdot}4).$$

Correction for pressure is a very simple operation. It is effected by a rule of proportion, thus:—as the mean pressure is to the observed pressure, so is the given volume to the volume required.

It has just been shown, that 100 cubic inches at 34°, corrected to mean temperature (60°), are equal to 105·4 inches, which being submitted to a second correction, namely, for pressure, the result is = 103·643 inches.

$$(29{\cdot}5 \times 105{\cdot}4 = \frac{3109{\cdot}30}{30} = 103{\cdot}643).$$

To insure still greater accuracy in weighing, measuring, and otherwsse investigating the properties and habits of gaseous bodies, and in comparing them with each other, it is sometimes necessary, not only to account for apparently trifling variations of temperature and pressure, but to ascertain the proportion of vapour present in the gas under examination, and to assign to it its proper share of the results exhibited by the mixture of which it forms a part.

From what has been already stated it is manifest, that a gas standing over, or in contact with, water, will be saturated with vapour, the quantity being always exactly proportional to the temperature. Under such circumstances, it may be very important to know what would be the volume and weight of the gas if it was perfectly dry. Calculations of this kind are greatly facilitated by the tables alluded to at page 255. As these tables are published in most of the popular treatises on chemistry, we deem it unnecessary to insert them here. We intend, however, to make such references to them as we think will enable the young student to comprehend their principles, and we are also anxious to point out the method of applying those principles in estimating the varied effects of evaporation, both in natural and artificial processes.

The density and elasticity of vapours vary with the temperature, and are proportional to each other. The temperature of a gas standing over water being known, it is easy to calculate the density and elasticity of the vapour mixed with it; and if the density or elasticity of the vapour is known, we can thence determine its temperature. The same remark is equally applicable to vapour when separated from a gaseous body, and to the vapour of other liquids as well as that of water.

We have just intimated, that inquiries of this kind are assisted by tables; the most useful of which is that founded on the experiments of Dalton and Ure, exhibiting the elastic force of aqueous vapour, expressed in inches of mercury, at every degree of temperature, from 0° to 325° F. To illustrate its use we subjoin a few extracts.

Temperature.	Elastic force, in inches of mercury.
32°	0·200
60°	0·524
180°	15·15
212°	30·00
252°	60·05

Here it will be observed, that the elastic force of the vapour of water at the boiling point (212°) is sufficient to support a column of mercury 30 inches in height; or in other words, it is exactly equal to the pressure of the atmosphere when the barometer stands at 30 inches. A vessel containing boiling water at 212°, say a steam-boiler for instance, which has an opening at the top, through which the steam can freely escape, is exposed to the same degree of pressure on the inside by the expansive force of steam, as it is on the outside by the weight of the atmosphere, so that these two forces balance each other. As it is well known that the mean pressure of the atmosphere is very nearly 15lbs. on every square inch, it follows, that the force of steam at 212° is also 15lbs. on the inch. The top and sides of the boiler, under the circumstances just described, being pressed outwards by the steam and inwards by the air, with equal and opposite forces, will, therefore, be precisely in the same state as if the boiler was cold, and the space above the surface of the water was occupied only by air.

Let us now imagine the aperture at the top of the boiler to be closed, and the temperature of the water within it raised from 212° to 252°. The elastic force of vapour at that temperature, it will be seen by the table, is equal to 60·05 inches of mercury; that is, its pressure against the top and sides of the boiler will be equal to 30lbs. on every square inch. The tendency of the steam to *burst* the boiler will not, however, be in the rate here mentioned, because the pressure of the atmosphere on its outside being equal to half the force of steam within, the effective force of the latter will be 15lbs. instead of 30lbs. on the inch.

The boiler being kept closed, and its temperature reduced to 180°, the force of vapour will be equal to 15·15 inches of mercury, rather more than half what it is at 212°. In this case, the resistance offered by the vapour to the external air is as 7½lbs. to 15lbs, and the tendency of the latter to *crush* the boiler is equal to 7½lbs. on every square inch of its surface, that being the difference between its own weight (15lbs.) and the force of vapour at 180° (7½lbs.).

A further reduction of the temperature of the boiler being effected, first to 60° and then to 32°, we perceive that the force of vapour at these temperatures is respectively 0·524 and 0·200 of an inch of mercury, which is equal in one case to about 4ozs., and in the other to 1½oz., on the inch. At 60°, therefore, the boiler will have to sustain a pressure from the atmosphere equal to 14lbs. 12ozs., and at 32°, 14lbs. 14½ozs., on every square inch of its external surface.

Let us now suppose the boiler to be open at the top, air having access to its interior, and mixing with the vapour at the respective temperatures of 180°, 60°, and 32°. By a reference to the experiments of Dalton, page 255, we learn that, under these circumstances, the quantity as well as the elastic force of the vapour, at every corresponding degree of temperature, will be precisely the same as it was when the boiler was closed; the density of the air diminishing in the same proportion as that of the vapour increases, and *vice versâ*, and their joint elastic force being equal to that of the external atmosphere.

We now advance a step further, our knowledge of the elastic force of vapour enabling us to determine its *density*, a term often used synonimously with specific gravity, and both signifying the *quantity* of matter of which bodies consist, compared with the *space it occupies;* a reference to some other body, which has been selected as a standard of comparison, being always understood. Gases and vapours are compared with atmospheric air, which, for convenience, is assumed as equal to unity. *(1 or 1·000).*

By very careful experiment it has been ascertained, by Gay-Lussac and Despretz*, that if a certain volume of dry air at 212° temperature, and a barometrical pressure of 30 inches, weighs 1000 grains, an equal volume of pure aqueous vapour at the same temperature and pressure will weigh 625 grains. Under these particular circumstances, therefore, the density of steam is expressed by its weight as compared with that of an equal bulk of air,—that is, as 625 grains to 1000 grains, or as 0·625 to 1.

But the relation thus subsisting between air and aqueous vapour, holds true only so long as certain specific conditions are observed. If the pressure be increased whilst the temperature remains constant, or, whatever change of pressure may occur, if the temperature be diminished, then will part of the vapour be condensed; the density of that which remains being proportional to its elasticity, and its elasticity dependent on its temperature.

These several points can be best illustrated by examples†.

Ex. 5.—100 cubic inches of gas (in contact with water), whose weight is 34 grains, temperature 60°, pressure 30; required, the *volume* and *weight* of the gas, if perfectly dry.

By the table at page 259, we find the force of aqueous vapour at 60° is equal to 0·524 of an inch of mercury, which, deducted from 30, leaves 29·476 inches for the elastic force of the gas. As the elastic force of the saturated gas (30) is to that of the dry (29·476), so are their respective volumes to each other, $\frac{100 \times 29 \cdot 476}{30} = 98 \cdot 253$ inches; or, as the elastic force of the moist gas is to that of the vapour, so is the volume of the former to that of the latter, $\frac{100 \times 0 \cdot 524}{30} = 1 \cdot 7466$ inches, which, being deducted from the whole volume, gives also 98·253 inches as the *volume* of dry gas.

The *weight* of the gas next claims our notice; and to ascertain that, we must first find the weight of the vapour present in it.

In computing the density of gases and vapour, the weight of a similar bulk of air is employed, 100 cubic inches of which, at 60° F., 30 B., according to the experiments of Prout, weigh 31·0117 grains. This result is, however, less than that obtained by Biot and Arago, who make it 31·1446 grains‡. For all practical observations we may assume 31·1 grains as sufficiently accurate. Until very lately, 30·5 grains was considered the weight of 100 cubic inches of air.

The relative densities of air and aqueous vapour at 212° temperature and 30 pressure, it has just been shown, are as 1 to 0·625. Now the weight of 100 cubic inches of air, at the same temperature and pressure, is 23·94 grains, and if we multiply that by 0·625, we have 14·96 grains as the weight of 100 cubic inches of steam. It is not, however, the weight of a given volume of vapour at 212°, but at 60°, that we have now to determine.

Bearing in mind that the density and elasticity of aëriform substances vary inversely as their volume, and that their density and elasticity are proportional to each other, if we know the specific gravity of any

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[*] Gay-Lussac, 0·6235 sp. gr.—Despretz, 0·6250 sp. gr. The last-mentioned result is usually adopted as most correct.

[†] One example is all we can introduce at present: others will more properly occupy a place in a future paper.

[‡] *Johnston's Chemical Tables*, page 7. British Association, 1836.

particular gas or vapour, at any one degree of temperature, we can thence ascertain it for every other; for it is obvious that, as the respective elastic forces at different temperatures are to each other, so are the densities at the same temperatures; subject, however, to correction for expansion and contraction, already explained as applicable both to gases and vapours.

To render this a little more intelligible, let us suppose a certain quantity of aqueous vapour, temperature 212°, pressure 30, to undergo such an enlargement of volume,—its temperature remaining constant,—that its elasticity shall be reduced from 30 to 0·524. If we look to Dalton's table, we shall perceive that the *tension* of vapour at 0·524 corresponds with 60° temperature; we must not, however, conclude that the *density* of the vapour, of which we have been speaking, is equal to that at 60°. Its elastic force may be the same, but that is the result of rarefaction, not of cooling; for we made it a special condition, that the temperature (212°) should remain constant. The density of the vapour, under the circumstances just described, bears the same relation to its former density that its elastic force at 0·524 does to its elastic force at 30. Hence ($\frac{625 \times 0{\cdot}524}{30} = {\cdot}010917$) its density is as ·010917 to 0·625.

Let it now be imagined that the temperature is reduced from 212° to 60°. In this case, the elastic force of the vapour will remain as it was before (0·524); for although there must of necessity be loss of tension by loss of heat, that will be compensated by diminution of volume, and in the same ratio that the volume has diminished, the density will have increased. Hence the formula already referred to, for finding the amount of expansion in aëriform bodies, is equally applicable in the present instance to determine their density, as consequent upon shrinkage; but it must be employed in an inverted form.

For instance,—Supposing that in a given volume of gas we wish to make the required correction for shrinkage in passing from temperature 212° to 60°, we must multiply by 508, the rate of expansion at 60° (480 + 28), and divide by 660, the rate of expansion at 212° (480 + 180). Exactly in the same ratio that the volume diminishes, the density increases, and by inverting the process just described, that is, by multiplying the density at 212° temperature, and 0·524 pressure, by 660, and dividing the product so obtained by 508, we arrive at the true density of aqueous vapour at 60°, which is $\frac{{\cdot}625 \times 0{\cdot}524}{30} = \frac{{\cdot}01091666 \times 660}{508} = {\cdot}01418306$.

We may now determine the weight of the dry gas, as demanded in Ex. 5; for if we multiply 23·94 grains, the weight of 100 cubic inches of air at 212°, by 0·01418306, the specific gravity of aqueous vapour at 60°, we find the weight of 100 cubic inches of the latter to be (1 : 0·01418306 : : 23·94 : 0·3395) equal to 0·3395 of a grain; and that is, in fact, the weight of the vapour present in 100 cubic inches of saturated gas at 60°.

In estimating the volume of dry gas, we stated the volume of vapour to be equal to 1·7466 inches, which is equivalent to 100 inches at 0·524 of an inch pressure corrected to 30 inches. The *volume* being thus corrected, it is necessary to make a similar correction for *weight*, by *which we learn that a cubic inch* of aqueous vapour, at 60° temperature *and 30 pressure*, weighs 0·1944 of a grain, and 0·1944 multiplied by

1·7466 gives 0·3395 of a grain. The last result, namely, 0·3395, must now be deducted from 34 grains, the weight of 100 cubic inches of saturated gas, and we have 33·66 grains as the weight of 98·253 cubic inches of dry gas.

We have now occupied the space allotted us. The principles which, at the hazard of being considered tedious, we have endeavoured familiarly to explain, will, in our next paper, be illustrated by showing their influence on the phenomena around us. and their application, in one way or another, to almost every process with which we are acquainted, whether natural or artificial.

SINGULAR PHENOMENA OF THE SENSE OF TOUCH*.

WEBER'S experiments on the Sense of Touch are extremely interesting and original. If, he observes, we touch the skin of a person, whose eyes are shut, with the points of a pair of compasses an inch apart, he becomes instantly conscious that his skin is touched in *two* places. If we gradually diminish the distance between the points, we shall at length arrive at a degree of approximation, with which the touch appears to be made by *one* body only, but which body the party touched describes as being a little longer in one direction than another; and it appears that the longer diameter corresponds with the line which joins the two points of the compasses. If the points be brought still nearer, this inequality in the diameters ceases to be perceptible, and the party cannot distinguish the sensation from one that is produced by a single point.

Now Weber has determined, by experiment, that different portions of the surface of the body vary considerably in accuracy of touch, as *measured by the distance at which the points of the compasses can be still distinguished from each other;* for it is found that parts endowed with great power of touch, will give notice of two points at a distance from each other so small, that when examined at the same distance by less sensible portions of the skin, these two points excite but a single sensation, and are by the touch erroneously judged to be but one. Thus the tips of the fingers, and the point of the tongue, were found to possess the most accurate sense of touch, for when the points were distant but half a Parisian line† from each other (counting from the inner surface of each point,) the feeling of two distinct points existed, and when they were within two-fifths of a line, although the person seemed to feel but one body, he nevertheless felt it to be longer in one direction than in another. The dorsum of the tongue was remarkably less sensible, for if the points were placed in a line parallel to the median, and were less distant than three lines from each other, they were not felt to be distinct. *Few persons, who have not tried similar experiments, will be prepared to credit the announcement of the very great difference which exists between the tactile accuracy of different portions of the skin.* On this subject the observations of Weber are quite novel, and open a new field for inquiry, not only to the physiologist, but to the practical physician and surgeon; for it is obvious that injuries or remedies applied to the skin must act with very different degrees of energy on parts so widely different in tactile sensibility from each other. Dr. Graves states that he has repeated many of the experiments of Weber, and can confirm his results. A little practice is necessary to be able to judge correctly concerning the variation of the sensation, when the points of the compasses approach each other and arrive within the *limit of confusion**.

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* *De Pulsa, Respiratione,* &c. WEBER—*Anatomical and Physiological Illustrations of the Pulse, Respiration, Hearing, and Touch. Leipzig,* 1834.

GRAVES—*Observations on the Sense of Touch, and Analysis of the Works of Weber on that Subject.—Dublin Medical Journal,* 1836.

† As is too often the case, the lovers of accuracy will here find a doubt as to the exact value of the *line* used. If it be the old Paris line, as is most probable, it will be near enough, for the purpose of these experiments, to consider it to be nearly equal to one-eleventh of an English inch. If it be the new or metrical line, it may be considered as exactly one-eleventh.

A few instances will suffice to prove the extent of the range of the limit of confusion, *when the points are placed on the same horizontal line.* n all these cases the observer operated on the skin of other persons.

	Lines.
Tongue, tip	1
Fingers, tips	
Tongue, margin at one inch from the tip	2
Toes, tips	3
Zygomatic bone	6
Forehead	
Hairy scalp	8
Arm, anterior surface	10
Back, middle	12
Loins	
Abdomen, on the side	
Arm, posterior surface	14
Scapula, near the upper border	24

The following were the results of experiments made by Weber on himself. The limit of confusion, in this series, is not confined to the distance at which the points of the compasses were perceived to be distinct from each other, but to that in which their direction, whether horizontal or perpendicular, could be decided upon.

	Lines.
Tongue, tip	0½
Finger-tips, inner surface	1
Lips, red parts	2
Fingers, inner surface of second phalanx	
Do. do. third do.	
Nose, tip	3
Metacarpal bones, inner side of extremities	
Tongue, dorsum, one inch from tip	4
Lips, parts not red	
Tongue-edge, one inch from tip	
Thumb, metacarpal bone	

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* *Limit of confusion*, a term proposed and adopted by Dr. Graves to express the distance at which the two touching points produce the feeling of one body only; longer, however, in one direction than another.

	Lines.
Apex hallicis	5
Buccinator	
Fingers, dorsum of second phalanx	
Hand, palm	
Eye-lid, surface	
Hard palate, centre	6
Zygomatic process, anterior surface	7
Fingers, dorsum of the first phalanx	
Metacarpal bones, outside of extremities	8
Lips, mucous membrane, close to gums	9
Zygomatic process, posterior surface	10
Forehead, lower part	
Heel, back part	
Occiput, lower part	12
Hand, back	14
Neck, beneath lower jaw	15
Scalp, vertex	
Patella	16
Sacrum	18
Acromion	
Gluteus	
Fore-arms, superior and posterior surfaces	
Leg, near the knee, and near the foot	
Foot, dorsum near the toes	
Sternum	20
Spine, dorsal, five superior vertebræ	24
Do. cervical, near occiput	
Do. lumbar	
Do. cervical, centre	30
Do. dorsal, centre	
Arm, at its greatest circumference	
Thigh do.	

After making an allowance for all probable inaccuracies in the experimental estimate of these distances, still this enumeration of them will afford ample matter for reflection; and the comparison of the relative distances which it enables us to make, will furnish abundant proofs, if any were wanting, of the wisest adaptation of parts to the functions they are appointed to discharge. Here is no unnecessary expenditure of tactile acumen, but a most rigid economy of the sense of touch; it is nowhere spread over surfaces indiscriminately, and without reference to their other physical qualifications. *This great difference was never before suspected to exist;* it was, indeed, known that the tops of the fingers, the tip of the tongue, and some other parts, enjoyed the sense of touch in a pre-eminent degree, and were capable of judging much more delicately concerning what they might be placed in contact with, than other portions of the body. This was attributed partly to habit, partly to their shape, and many laid great stress on the facility with which these extremely moveable parts could be adapted and applied to bodies undergoing examination. Now, for the first time, has it been proved by Weber, that, quite independently of all these extraneous circumstances, the skin itself varies in the intensity of its tactile power; and that this arises not from the mere varying thickness of the epidermis, and general delicacy of conformation in the cutaneous tissue, but from an original difference in its organization. All these facts tend strongly to overturn the common hypothesis, that the sense of touch is diffused throughout the whole texture of the skin, and render it much more probable that it is performed only by certain organs, extremely minute, and comparable to points merely, but differing greatly in their mode of distribution, being crowded together, and very numerous in some parts of the skin, in others more sparingly present, and in some, very thinly scattered. On this supposition alone, can we account for the signal differences in tactile discernment, which different portions of the skin exhibit.

The researches of M. Breschet, to which the attention of the English public was first drawn, by the able analysis of Dr. Costello, published in the "Dublin Medical Journal," for September 1835, have rendered it certain that the sense of touch is performed by a less simple apparatus than was generally imagined. M. Breschet considers that the nerve parts with its neurilema at the derma, as the optic nerve does in entering the sclerotic, and that the projecting papillæ take a new envelope from the outer surface of the derma; that the mere nervous pulp does not, of itself, constitute the sense of touch, but that, as in the sense of hearing or of sight, there is an apparatus, all the parts of which must be in unison to be perfect. If any one of the five constituent parts be wanting, touch cannot be exercised, and the derma, neurilema, and proper epidermic membrane, are to the papilla, what the complicated apparatus of sight and hearing are to the optic and acoustic nerves. The analogy goes farther, for the optic and acoustic nerves, on entering the structure of the eye and ear, undergo the same change as the tactile nerve entering the derma, though there is this difference,—the two former remain in their cavities, light penetrating to the one, and sound to the other; but the nerve of touch must advance, as it were, to meet impressions. The following very curious phenomenon is recorded by Weber:—"If the points of a pair of compasses, distant from each other one or two lines, applied to the cheek, just before the ear, be then moved successively to several parts of the cheek, we shall find, on approaching the angle of the mouth, that the points will appear to recede from each other; this is produced by the great difference of tactile power in these parts. It is a general law, that the more sensitive portions of the skin regard any two points as farther asunder from each other, than equidistant points appear to be to a less sensitive portion. The same experiment may be tried by holding together the extremities of the forefinger and thumb, and then passing the tips of both in a line from the ear to either the upper or the under lip; as they approach the latter, they will feel to the cheek as if they were becoming more and more distant from each other."

Another fact was observed by Weber:—"If the points of the compasses be applied to two contiguous surfaces enjoying the functions of voluntary motion, they will appear to be much more distant from each other, than when they are applied to one only of these surfaces.

Thus, if the points are distant half a line, they are not perceived to be distinct when applied to one lip, but when one point is applied to the under lip, and another to the upper, they are at once felt to be two."

Another very remarkable conclusion announced by Weber deserves consideration:—"Apply the compasses to two portions of the skin, differing from each other remarkably, either in structure, in function, or in the use habitually made of them, and the points will appear to be more clearly and distinctly felt than when they are applied to one and the same surface, even though it be the more sensitive of the two; thus the points, when in contact, one with the inner surface, and the other with the red outer surface of the lips, appeared much more distant from each other than when they were in contact with the red outer surface only, which has much greater tactile powers than the inner surface. The same observation applies equally to neighbouring surfaces, which differ much from each other in tactile power, viz., the margin and the dorsum of the tongue, the volar and the dorsal surfaces of the finger-ends," &c. One result of Weber's experiments is of great importance in a physiological point of view:—"The tactile powers of any part of the skin are not, as is generally imagined, directly proportioned to its sensibility; thus the mammæ are easily tickled, and capable, when irritated, of producing great pain; in these respects they exceed any portion of the trunk, and yet the skin of, or round the nipples, is but very indifferently endowed with the faculty of touch, properly so called. Indeed, the same remark applies to the arm-pits, the flanks, the soles of the feet, &c., and all ticklish parts of the skin in general, for they are possessed of a comparatively slight power of discriminating objects from each other by means of the touch. Who was ever made to laugh by tickling the points of his fingers? and yet they are possessed of a tactile accuracy far exceeding that of any other portion of the skin!"

This is a very curious subject of inquiry, and one not yet investigated. The reason of the matter is sufficiently obvious,—parts endowed with the greatest tactile acumen are necessarily much exposed, being so placed as to be brought with the greatest facility into contact with external bodies, consequently, if so disagreeable a sensation as that arising from tickling were easily induced by this contact, such parts would be almost useless as organs of touch. The experiments of Weber, considered with reference to the researches made by Breschet on the structural anatomy of the skin, render it extremely probable that the sense of touch, properly so called, resides in a peculiarly constructed apparatus, supplied with certain ramifications of the cutaneous nerves, while the function of sensation, comprising the power of perceiving painful or pleasing impressions, is much more generally diffused, and is the result of a simpler organization. In fact, although the internal, mucous, fibrous, and serous surfaces, and the parenchyma of the different organs, are all capable of becoming actually painful, particularly when inflamed, yet it is very doubtful whether the sense of touch, properly so called, is ever exercised by these parts. No foreign substance is ever distinctly felt by the sense of touch in the stomach and bowels; a sensation, painful or pleasing, is indeed excited by some matters immediately after they are swallowed, but all consciousness of their presence, by means of the sense of touch, soon ceases, and it cannot be again recalled by the utmost exertion of the will. A foreign substance lodged in the alimentary canal, or in the trachea, may give rise to the greatest possible degree of irritation; but though it thus acts upon the nerves of the parts immediately in contact with it, these nerves convey no idea to the sufferer of the shape or size of the body, or of any other of its physical qualities, concerning which we receive information through the medium of the sense of touch.

Weber's observations on the comparative tactile energy of the different portions of the trunk of the body are extremely curious, and disclose a very remarkable difference between the sense of touch in the trunk and in the extremities. In the latter, where the points of the compasses are placed across the axis of the limb horizontally, they are much more accurately distinguished than when they are placed in the longitudinal direction, or parallel to the axis of the limb (vertical); in other words, the *limit of confusion* is much sooner attained in the vertical than in the horizontal position of the points. Now, in many parts of the trunk the contrary obtains, and the vertical position is more accurate than the horizontal; this singular difference Weber explains by the different manners in which the nerves supplying the extremities and the trunk are distributed; the branches of the former generally run nearly parallel to the axis, while those of the latter pursue, in most cases, a transverse course. All parts of the trunk do not exhibit this difference. Whether this explanation be or be not admitted, the fact is undoubtedly true. Weber proceeds to show that motion, whether it be of the touching organ, or of the body to be touched, greatly augments the clearness and accuracy of the perception,—a fact too familiar to require any elaborate illustration. As to the *idea of direction* which we derive from the sensation imparted to the skin by any minute substance, he justly observes, that it is always judged to be perpendicular to the surface of the skin at the point of contact. Of this there can be no doubt, and here we have a very striking analogy between the senses of vision and of touch, for it is a primary law, that rays of light impinging on the retina always produce a sensation, (*i.e.* are seen in a direction) perpendicular to the retina at that point; it would be well worth while examining whether the same law of perpendicularity extends also to the ear. In the case of the eye, this law is strikingly useful, as it enables many rays, originally diverging from the same luminous point, all to create a sensation in the same direction, although in converging they strike the retina from very different directions; in the eye all these perpendicular lines intersect at a common point, thence called the centre of visible direction, and this result, derived from the spherical shape of the retina, is attended with the most important consequences. No one has as yet attempted to investigate the question, whether any similar provision or contrivance exists with regard to the lines of direction, to which each part of the auditory nerve receiving vibrations refers sound; any given point of the hearing surface of the acoustic nerve receives impulses from the vibration essential to this sense, conveyed either through the fluid of the vestibule and semicircular canals, or through the solid bone surrounding the *cochlea;* the question arises, whether vibrations excited originally by the sounding body arrive by different routes simultaneously at the same point of the nerve, so as materially to reinforce and strengthen each other. Is there in this case any provision made to prevent vibrations, which arrive in different directions, from interfering with each other, with reference to the sensation they produce? Or are both, as impinging on a common point, referred to one common direction? If this were the case, the analogy between the perceptive properties of the retina and the auditory nerve would be perfect, and nothing would remain to the philosophical examiner of the mechanism of the sense of hearing, but to discover what relation these lines of common direction bear to the surface of the auditory nerve, and to each other; are they, as in the case of the retina, perpendicular to the nervous surface? and in what manner are they so arranged, that, in consequence of the shape of that surface in the convolutions of the internal ear, each line of direction resulting from the vibration communicated to any point, may be parallel to the various other lines of direction which result from vibrations, simultaneously communicated to all other points of the nervous surface?

These are extremely difficult questions, but it is by no means improbable that they may be hereafter satisfactorily resolved. But, to return to the sense of touch:—in some parts of the surface an exception seems to occur to the general rule of perpendicularity; thus, when a hair of the head is pulled, we can judge perfectly well of the direction in which it is pulled. The most obvious explanation of this fact, which refers the discrimination of the line of traction to the bulb of the hair, Weber proves to be erroneous, and he shows that we judge of the direction in which the hair is pulled, by means of the muscles called into exertion to counteract the pull, and keep the head steady during its continuance. If these muscles be not called into play, which is the case when the head is held steadily by the hands of one person, while another, by surrounding the point in which the hair is pulled, with a firm pressure made by the fingers, thus prevents the least motion in the enclosed portion of the skin, then, no matter in what direction the hair is pulled, the person cannot judge of it.

Weber has also made some valuable experiments on the faculty the skin possesses of estimating and comparing different pressures made on its surface, and of its faculty of perceiving heat and cold. If both the right and the left hand of the same individual are supported on cushions, and he keeps his eyes shut, while unequal weights are placed one on each hand, he will, if the difference between the weights is considerable, be able to tell on which hand the heavier lies; slight differences of weight cannot be thus estimated, but they at once become perceptible if the hands be raised from the cushions; the muscles that now support the weight give great assistance in estimating its amount. Thus we judge of the weight of any heavy body, partly by the pressure it produces against our surface, but chiefly by the quantity of muscular force it requires us to use in lifting or sustaining it. Weber has ascertained that in most men the left side of the body and the left extremities enjoy a more accurate perception of weight than the right, so far as weight is estimated by pressure; of fourteen different persons experimented on, the left side of the body and the left extremities were found to be more sensible of weight, measured by pressure, than the right, in eleven; in two the contrary was observed, and in one only no difference between the sides could be detected. He offers no satisfactory explanation of this very remarkable and hitherto unobserved phenomenon. It is obviously of some value in marking an original difference between the nervous power of the right extremities and right side of the trunk, as compared with those of the left, a difference which favours the idea, now indeed generally admitted, that we cannot explain the circumstances of man being right-handed and right-footed, except on the hypothesis of an original difference in the vital powers of the right and left halves of the body.

Experiments made for the purpose of comparing the energy of the perception of heat and cold possessed by different parts of the body, are attended with obvious difficulties; thus, if the surface of the substance applied to the body be not exactly of the same extent in two cases, the result is not to be relied on, for, *cæteris paribus*, a larger body will feel hotter or colder than a smaller, and that in a very remarkable degree. Thus, let one vessel contain water heated to 98°, and another water at 104°; now if the finger be placed in the latter, and the whole of the other hand be immersed in the cooler, we shall be led to form a wrong judgment, and will pronounce the water at 98° to be hotter than that at 104°! In some cases a similar error was made when the difference of temperature amounted to 8°, the hotter being at 106°. If the parts are kept a good while immersed, the person sometimes becomes sensible of his error, and judges rightly.

Weber has discovered a very remarkable fact, that, *in most persons, the left hand is more sensible of heat or cold than the right.* Thus, when the hands of a person lying in bed, and of exactly the same temperature, were plunged in separate vessels of hot water, the left hand was believed by the person to be in the hotter medium, even though the water it was in was really one or two degrees colder than the other. Weber has rendered it highly probable that the greater sensibility which the left hand undoubtedly possesses in perceiving changes of temperature, is owing to the circumstance of its being covered, particularly on its palm, by a thinner epidermis, in consequence of being less used. Nothing is more striking than the accuracy of the skin in giving notice of changes of temperature, for a difference of one-third of a degree is detected clearly when the hand is immersed repeatedly and successively in two vessels of water, differing that quantity only in temperature. The skin detects best very minute changes of temperature when the medium examined does not fall short of, or exceed very considerably, the usual temperature of the body. Water at 98° can be much more certainly distinguished by the hand from water at 100°, than can water at 120° from water at 118°. As the ears perceive best a difference of tone in sounds, neither too acute nor too grave, nor immoderately loud, so the skin judges with most accuracy of such medium-temperatures as produce no very violent or painful effect on its nerves. Weber is of opinion that the perception of temperature imparted to each nervous extremity in the skin, goes to unite itself to, and strengthen, simultaneous impressions in the other ramifications of the same nerve, thus producing, by the conflux of a great number of impressions, a much stronger result and effect. This, at least, is certain, that a large surface conveys much stronger impressions than a small one, and estimates changes of temperature with greater delicacy. Thus, if we place the fore-finger of one hand in water at 104°, and plunge the whole of the other hand into water at 102°, the latter will appear to us to be the warmer. If we plunge the finger successively into vessels containing hot water, we are unable to perceive very minute differences of temperature, which at once become perceptible when we use the whole hand. Nay, water, which can easily be borne by a single finger, will appear intolerably scalding to the whole hand. With regard to the power the skin possesses (by means of touch and its modifications,) of comparing together two different temperatures or weights, various and multiplied experiments prove that this power is exercised with the greatest success when the perceptions compared are not simultaneous but successive. It is the same with the smell, taste, and hearing; apply to the tongue, by means of camel-hair pencils, small portions of an acid and of a sweet substance; if the application of both be in quick succession, their taste is accurately distinguished and appreciated; but if they be applied simultaneously, the result is a less vivid perception of either, and a blending, as it were, together of the acid and the sweet. A similar result is obtained by applying the mouths of phials containing two different, but strongly odoriferous substances, to the nostrils; and musicians have long ago remarked, that when we wish to compare together two notes, it is done with much more accuracy by striking them in quick succession, than by striking them simultaneously. Vision appears to present an exception to the law which governs the other senses; for if we want to compare the lengths or the colours of any two lines, we place them close together, and look at them at the same moment. As Weber well remarks, however, the exception is here apparent only, for the truth is, *that we see nothing with perfect accuracy except its image falls on the retina at the extremity of the optic axis;* consequently, on examining two lines close beside each other, although we think we examine them simultaneously, yet we do not do so; our examination and comparison is made by causing the image of each to occupy the extremity of the optic axis several times in very rapid succession. The change in the position of the eye is here so slight, and is performed with such ease, that we are unconscious of it.

Weber made many experiments on the accuracy of the sense of weight; of course this sense is more developed in some individuals than in others, and is capable of being rendered more exact by practice. Men accustomed to estimate weights by poising them in their hands, will distinguish perfectly between two differing by a thirtieth part only. In comparing two weights, one is poised, and then instantly the other, *in the same hand;* the intervention of a few seconds between the poising of the first and of the second, does not prevent their accurate comparison. The interval may amount to twenty seconds, and a just estimate still be made; but when it amounts to forty seconds, all accuracy is lost. The sight enjoys a still more accurate power of discrimination than the sense of weight, for a well-practised eye will distinguish between two lines, 100 and 101 *lines* long, respectively—in other words, will discover a difference amounting to $\frac{1}{100}$th part of the whole. According to the experiments of Delezennius, quoted by Weber, the sense of hearing is still more accurate, for a well-practised musical ear will distinguish between two sounds differing from each other only $\frac{1}{320}$th, calculating the number of vibrations the sounding bodies make in a given time.

A line can be perceived to be longer than another, even when an interval of fifty or sixty seconds elapses between looking at the first and at the second, provided that the lines differ $\frac{1}{11}$th in length. If they differ only $\frac{1}{21}$st, an interval of thirty-five seconds may elapse without destroying our judgment; but if it be longer, our judgment becomes incorrect. When the difference between the lines amounts to $\frac{1}{50}$th only, an interval of three seconds between the examination of each is the longest that can be allowed, without interfering with the correctness of the comparison.

Dr. Graves concludes his able analysis of Weber's Treatise on the Touch, by a summary of the chief conclusions which the German philosopher has arrived at:—"We have a well-established and definite idea of the distance of some parts of our bodies from others. Thus, we feel the distance of the finger-points from the wrist, and we remember that distance. It is the same with the arms as far as the elbow, and with the foot. These are all lengths which are firmly imprinted on the mind, and consequently there is a physiological reason for using them, as mankind have always done, as standards of measurement. When any two points on the surface of these parts are touched at the same time, we can with our eyes shut, and by means of the sense of touch alone, guess with great accuracy the distance the touched points are from each other, provided the points are situated somewhere near the sides or extremities of these parts, as at the tips, or on the sides, of the fingers. Here two points will be perceived to be distinct at distances much less than half an inch; but if the points be situated elsewhere, as on the back of the hand, then, although they may be distant from each other half an inch, they will scarcely be felt as distinct, provided the line joining them is parallel to the longer axis of the part; when it is transverse, the perception is much clearer, and exists at smaller distances.

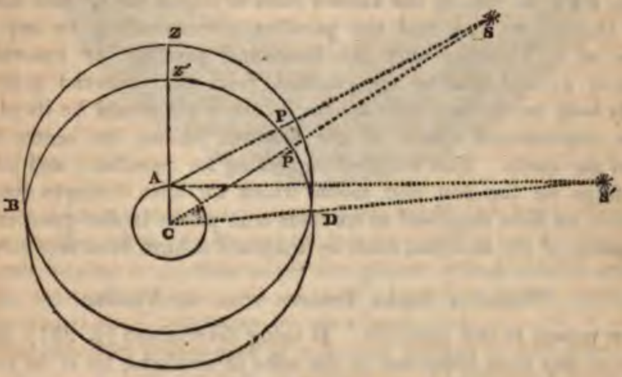
"The discovery that two equi-distant points of contact on the same surface excite very different ideas of the distance between them, according as the space lies *lengthways* or across the limb, is one of the most striking and important that Weber has made, and one that can be most readily verified by experiment."

A POPULAR COURSE OF ASTRONOMY.

No. XI.

PARALLAX.

THAT sphere of the heavens to which we refer the positions of all the heavenly bodies, and in degrees of which sphere we estimate their longitudes and latitudes, declinations and right ascensions, measures which serve to ascertain their positions on it,—this imaginary sphere of the heavens is *not*, in reality, the sphere which we *see* around us when we look out upon the heavens. It has its centre in the centre of the earth, whereas that which we actually see has its centre in the eye of the observer, who is placed on the surface of the earth. Now it has been before shown, in the course of these pages, that because of the enormous distance of the region of the fixed stars, they appear to us precisely in the same position, on the sphere which we actually see from the earth's surface, as they would upon a sphere whose centre was the centre of the earth, could the eye look upon it from thence. This conclusion, which is perfectly true with regard to the fixed stars, is not, however, true in reference to the sun and planets, forming that system of the universe, which, by reason of our more immediate relation to it, we call our own; and especially it is not true in reference to the moon. All the bodies of our system are infinitely nearer to us than the fixed stars, and they do not appear upon that visible sphere of the heavens upon which we see them, as they would appear upon a sphere having the earth for its centre, could we see them from thence.



Let A be the position of the observer at the earth's surface, and B Z D the sphere of the heavens which he sees. Also, let C be the earth's centre, and B Z′ D the sphere of the heavens which he would see if his eye were there. Let S be any heavenly body not at an infinite distance as compared with A C. Join S A and S C, then will P and P′ be the points where S will appear on the spheres B Z D and B Z′ D respectively. Also, if C A be produced intersecting the spheres in Z and Z′, then will these points be the apparent zeniths of the two spheres, and

Z P and Z′ P′ will be the apparent zenith distances of S. Now Z P contains as many degrees as there are in the angle Z A P, and Z′ P′ as many as there are in Z′ C P′, and Z A P is greater than Z′ C P′ (*Euclid*, 1—16), therefore Z P is greater than Z′ P′. That is, the apparent zenith distance of S is not the same as it would have been if seen on a sphere having its centre at the earth's centre, instead of at the earth's surface, or its place on the great imaginary sphere of the heavens is not the same as its place on the sphere on which it is actually seen; the difference between these is called the PARALLAX. In the case we have supposed, it is manifestly equal to the difference of the arcs Z P and Z′ P′, or the angles Z A P and Z′ C P′. Now, the difference of these angles is (*Euclid*, 1—32) the angle A S C; the angle A S C is, therefore, the parallax. Also, knowing what is this angle A S C, and observing Z P, we can tell what is Z′ P′; that is, observing the place of S on the apparent sphere of the heavens, and knowing the parallax, we can tell what is its place on the true astronomical sphere of the heavens. It is clear that the value of the parallax A S C is greater or less as S is more distant from or nearer to the zenith. It is greatest when S is at S′, 90° from the zenith, or in the horizon; and it vanishes altogether when S is in the zenith.

The parallax of a heavenly body when in the horizon, is called its HORIZONTAL PARALLAX. Join S′ A and S′ C, then is A S′ C the horizontal parallax of S, it is equal to the angle under which the semi-diameter, C A, of the earth would appear to an observer from S. Knowing the horizontal parallax of S, we can tell its actual distance, C S, from the centre of the earth, for in the triangle C A S′, whose angle A is a right angle, we know C A, the radius of the earth, and the angle C S′ A, whence we may find C S′ or C S*. Also, knowing C S and C A, and the apparent zenith distance, Z A S, we can, by the known rules of trigonometry, find the angle A S C†; that is, we can find the parallax corresponding to any zenith distance of S, knowing only the *horizontal* parallax, or knowing the distance of S; and thus we are enabled, from the observed place of a heavenly body on the apparent sphere, to tell what would be its place on the true astronomical sphere of the heavens, having its centre in the centre of the earth. This is called correcting for parallax; and all those observations on the sun and moon which we have hitherto described, and which we have supposed to fix their true places in the great astronomical sphere of the heavens, must be imagined to have been thus corrected.

WHERE A SOLAR ECLIPSE WILL BE VISIBLE.

WE now return to the question. It being determined (p. 221) that the moon is at any time immersed in the cone of sunlight, let it be required to determine *where* at that instant of time is the sun's apparent eclipse, and how much eclipsed at each place.

Let M and S represent the true places of the centres of the moon and sun at the given instant, as found on a celestial globe. From the

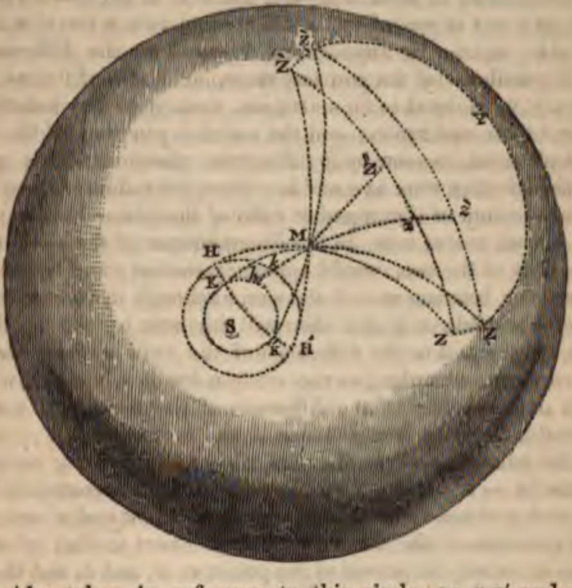
* $\mathrm{CA} = \mathrm{CS'}\,\mathrm{Sin.}\,\mathrm{AS'C}$

$$\mathrm{CS'} = \frac{\mathrm{CA}}{\mathrm{Sin.}\,\mathrm{AS'C}}$$

† $$\frac{\mathrm{Sin.}\,\mathrm{ASC}}{\mathrm{Sin.}\,\mathrm{ZAS}} = \frac{\mathrm{CA}}{\mathrm{CS}}$$

$$\therefore\ \mathrm{Sin.}\,\mathrm{ASC} = \frac{\mathrm{CA}}{\mathrm{CS}}\,\mathrm{Sin.}\,\mathrm{ZAS}$$

centre S, describe a circle H *k* H′, having for its radius the sum of the semi-diameters of the sun and moon. Take H M and H′ M, arcs of a great circle of the sphere, equal to the difference of the horizontal parallaxes of the sun and moon, and let them meet the circle H H′ in the points H and H′; continue these arcs to Z and Z′, so that M Z and M Z′, may each equal 90°. Through any point, *k*, between H and H′ draw an arc *k* M of a great circle, and produce it to *z*, so that M *k* may equal M H × sin. M *z*, and let the dotted line Z *z* Z′ be the locus of points similar to *z* found corresponding to each of the points in H *k* H′; then, at all the places whose zeniths are in the line Z Z′, the discs of the sun and moon will be seen just in contact with one another. If K *h* K′ be any other circle less than H *k* H′, but having the same centre S, and if from the centre M an arc H K K′ H′ be described, cutting this circle in K and K′, and if the points



,Z ,Z′ and *z*′ be taken in reference to this circle, as Z Z′ and *z* were in respect to the other; moreover, if the line ,Z *z*′ ,Z′ be the locus of all the points similar to *z*′, then at all the places whose zeniths are in this line, the sun will appear to be eclipsed by a quantity, H K, equal to the difference of the radii of the circles H *k* H′ and K *h* K′.

The following is the proof of this easy method of constructing for a solar eclipse. Let us first take the zenith *z*′; it is required to prove that at the place whose zenith is *z*′ the sun appears eclipsed by a quantity equal to the difference of the radii of the circles H *k* H′ and K *h* K′. The zenith distance of the moon being at this place M *z*′, if we call its ***horizontal*** parallax H, it will, by the last article, be depressed by parallax through a space equal to H sin. *z*′ M. Moreover, if H′ be the ***sun's*** horizontal parallax, the space through which it will be depressed by parallax at *z*′ will be H′ sin. *z*′ S. Therefore, the quantity by which the centres, of the sun and moon will be brought (vertically) nearer to one another by parallax will be $H\ \mathrm{sin.}\ z'M - H'\ \mathrm{sin.}\ z'S$; that is, this will be the angular space, measured on the vertical, by which the moon will be made more nearly to intervene between the spectator and the sun, than it would if the spectator were at the earth's centre instead of at the supposed point on the earth's surface. If, therefore, we suppose the apparent position S of the sun to be unchanged by parallax, and the moon M to be moved down on the vertical $z'M$ which passes through its centre, through a space equal to $H\ \mathrm{sin.}\ z'M - H'\ \mathrm{sin.}\ z'S$, then will the *relative* positions of the centres of the two be the same as they appear to be to a spectator whose zenith is z'. Now, since the centres of the sun and moon are, by supposition, exceedingly near to one another (the moon being partly or wholly within the cone of sunlight); moreover, since H' is very small, it follows that $H'\ \mathrm{sin.}\ z'S'$ is very nearly equal to $H'\ \mathrm{sin.}\ z'M$. So that, $H\ \mathrm{sin.}\ z'M - H'\ \mathrm{sin.}\ z'S$ is very nearly equal to $H\ \mathrm{sin.}\ z'M - H'\ \mathrm{sin.}\ z'M$, or to $(H - H')\ \mathrm{sin.}\ z'M$.

But Mh was supposed to be taken equal to $MK\ \mathrm{sin.}\ z'M$, and MK is equal to MH, which, by supposition, is equal to the difference of the horizontal parallaxes of the sun and moon, or to $H - H'$; thus, then, Mh, or $MK\ \mathrm{sin.}\ z'M$, is equal to $(H - H')\ \mathrm{sin.}\ z'M$, or to the *relative* vertical depression of sun and moon; and the *relative* positions of the centres of the sun and moon, as seen by an observer whose zenith is z', are the same as though they were at S and h. Now, the radius of the circle HkH' is equal to the sum of the apparent radii of the sun and moon; if, therefore, the moon's centre were in the circumference of the circle HhH', her limb, and that of the sun, would appear in contact; and her centre actually interposing between us and the sun, as though it were in the circumference of the circle KK', her disc will lay over or overlap that of the sun, by a space equal to the difference of the radii of these circles, or to the sum of them, according as the circle HkH' lies between M and S, or beyond S, or according as the difference of the horizontal parallaxes is less or greater than the true distance of the centres.

At the points $Z_{,}$ and ${}_{,}Z'$, the arcs $Z_{,}M$ and ${}_{,}Z'M$ being each 90°, the expressions $(H - H')\ \mathrm{sin.}\ Z_{,}M$ and $(H - H')\ \mathrm{sin.}\ Z'M$ became each $H - H'$; now MK and MK' are each equal to $H - H'$, so that K and K' are the corresponding positions of the moon's centre in respect to that of the sun at S; and the same may be proved of the points H, H', and k, and the line ZZ' as has been proved of K, K', h, and the line $Z_{,}\,{}_{,}Z'$, with this only difference, that to all the points of the earth's surface whose zeniths are in the line ZZ', the discs of the sun and moon only just touch one another, their apparent distances (the radius of the circle HH) being equal to the sum of their semi-diameters, whilst to those who sezeniths are in the line $Z_{,}\,{}_{,}Z'$, the disc of the moon overlaps or eclipses the disc of the sun, by a quantity which was shown before to equal the difference of the radii of the circles HkH' and KhK'. If there be described an arc $Z'YZ$ of a great circle from Z to Z', that arc will be everywhere distant 90° from M, and to every place of the earth's surface whose zenith is in that line, the sun will, at the time supposed, be rising or setting eclipsed, (since he will be at a distance of 90° from the zenith), and to every point whose zenith is included between this arc and the arc ZZ', the sun will appear at various distances from the zenith of which the perpendicular distance $Z'M$ is the least, but in all these positions more or less eclipsed.

Such are the circumstances under which an eclipse of the sun is visible at any instant of the time during which the moon lies more or less within the cone of sunlight, and these being ascertained for all the different periods of the immersion, the eclipse is completely determined.

THE APPARENT MOTIONS OF THE PLANETS.

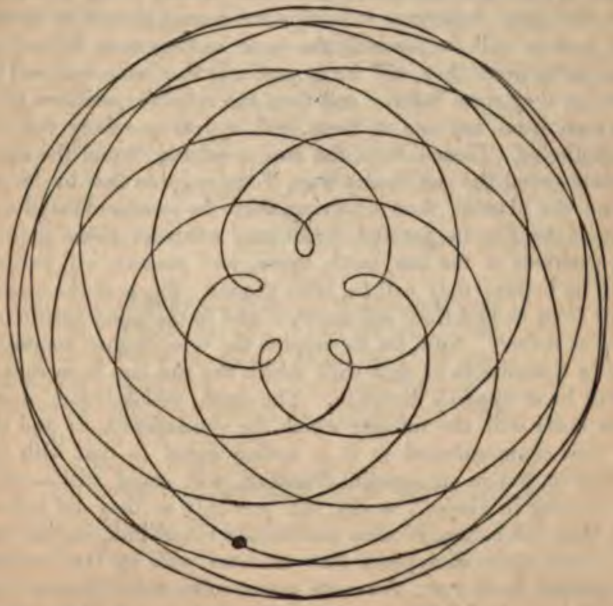
EVERY BODY is acquainted with the outline of the system of the universe, as described in our elementary books of astronomy. It is a system of such exceeding simplicity, that even at the very earliest period when our attention is directed to it, we at once understand it, and the slightest effort is sufficient in after years to replace it in our memories, and to present it to our imaginations. A sun fixed at rest in the centre, planets wheeling round it, each in its proper order, and in its own periodic time; Mercury, Venus, the earth, Mars, Ceres, Pallas, Juno, Jupiter, Saturn, Uranus;—the earth accompanied by one moon, Jupiter by a group of four, Saturn by seven, and surrounded by a ring, Uranus by six,—these facts, remembered probably in connexion with the period and distance of each planet, constitute the science of astronomy, as it commonly exists in men's minds—and a prodigious amount of knowledge it is, if we estimate it by the years of laborious observation, the multitudinous theories, and the complication of systems, out of which it has resulted. This accumulation of labour, and the variety of these abortive systems are, however, things which we find the greatest difficulty in reconciling with the exceeding simplicity of the system itself. And from the time when we first looked at a chart of the solar system, gazed at an itinerant orrery, on learned those rhymes which make the sublime doctrines of Copernicus easy to the comprehension of children, and embody our first conceptions of the harmony of the spheres—even from these early and happy days, we accustom ourselves to wonder that so simple a thing as the Copernican system should have been so long in being found out. In the eight or ten white circles which we see marked upon our maps of the solar system, we look in vain for the difficulty; and thus one of our earliest emotions of learned contempt is accustomed to connect itself with the names of such men as Plato, and Aristotle, and Ptolemy, who taught that the earth was the centre of the universe, and that the planets revolved in various and complicated ways about it,—and the weak invention of one Tycho Brahe, who thought that whilst the other planets revolved round the sun, the sun itself, with these, revolved round our planet. *Our* acquaintance with the solar system is derived from books and engravings; those men got *theirs* from an actual survey of the heavens; and were not their knowledge of practical astronomy otherwise ascertained, these very theories, false and complicated as they are, would be sufficient to prove it to have been most extensive and accurate. The fact is, that the motions of the heavenly bodies, and especially of the planets, simple as they are in reality, are, in *appearance*, complicated in the highest possible degree.

Let us suppose that, throwing aside books, and resolute to discard previously formed opinions, until we shall have ourselves verified them—provided, moreover, with one of Troughton's best micrometers, fitted to a good telescope, by Tully, and a transit instrument, by Sims, we undertake to examine the apparent motions of one of the planets, Mercury for instance, in the heavens.

We shall very soon perceive that there are certain periods when this planet appears for several successive days not at all to move its place in the heavens; that at other times he moves rapidly forwards eastward, or in the direction of the sun's motion; and that at other times he retrogrades for a short space, or moves westward, and then returns again to his forward course. Moreover, that sometimes he moves faster in longitude than the sun, and sometimes slower; so that at one time he gains upon, and at another is gained upon by him. Now, in this way he will be seen, if to the west of the sun, after a time so to have gained upon him as actually to have overtaken him, to have passed him, and to have passed considerably to the east of him, rising before him in the morning, and setting before him in the evening; and thus, being what is called a morning star, he will continue to gain upon the sun, getting further from him to the eastward, for about fifty-eight days, and then the sun will begin to gain upon him, and in fifty-eight days more will have overtaken him, will pass him, and continue to leave him further behind for another fifty-eight days, when the planet will begin to gain on the sun as before.

But let us follow these motions yet more accurately. Suppose that on the 1st of January Mercury was at one of his stationary points, and his apparent diameter measured by means of the micrometer, and his longitude marked on a chart; on the 10th of January he will have appeared to move forward in longitude through about 7°, the sun having moved somewhat more; and if his apparent diameter be now measured, it will be found to have become considerably less than it was, so that his distance must have become greater: taking, moreover, his first distance from the point supposed to represent the earth's place on the chart, and his present distance, in the inverse ratio of his apparent diameters, and giving him his proper longitude, we shall find his place on our chart at this second period. The 10th of February will find him advanced about forty more degrees eastward; in which time he will have considerably gained upon the sun; and his diameter, if examined, will be found to be still less than at the last observation; so that he must yet further have receded from the earth: his new position will be marked on the chart like the last. This diminution of his apparent diameter, and consequent increase of his actual distance from the earth, will continue until about the 22nd of February, when it will attain its maximum, and the planet will, in the intervening twelve days, have advanced at least 15°, still gaining on the sun. On the 10th of March the apparent diameter will have diminished to about what it was on the 10th of February, and the planet will have moved, in the intervening twenty-nine days, through about 44° of longitude, greatly gaining, as before, on the sun. The 20th of March will find him advanced 18°, and still rapidly gaining on the sun, and his apparent diameter will have considerably increased: the next eleven days completing the month will only find him advanced 12°, still gaining, however, upon the sun, but greatly less than in the preceding ten days. With this diminution of his motion forward in the direction of the signs, his motion directly towards the earth will, however, be greatly increased, as will be indicated by a rapid increase in his apparent diameter. From the 1st to the 10th of April the planet will only have moved through five degrees of longitude; on the 10th, his apparent diameter will have become again what it was on the 1st of January; and he will appear stationary in the heavens,—that is, stationary in longitude; for a daily and exceedingly rapid variation in his apparent diameter will indicate that he is rapidly approaching the earth. After two or three days, it will be manifest that the planet is now going backward in the heavens, or towards the west; and by the 20th of April he will have retrograded through at least 5°, and his apparent diameter will then be the greatest: this retrogradation will continue until about the 28th, when he will, on the whole, have retrograded through from 9° 22′ to 15° 44′. Between the 20th and 28th his apparent diameter will have diminished, precisely in the same order and proportion in which it increased from the 10th to the 20th. After the 28th, having appeared stationary for a few days, he will begin to advance, and will go through precisely the same changes as he commenced on the 1st of January.

The planet was gaining upon the sun from about the end of January to the end of March. During this period he will have passed from his greatest distance westward of the sun to his greatest distance eastward, and will thus have passed him in longitude, or at some intervening time had the same longitude with it: he is then said to be in conjunction with the sun.



If the places of the planet at a series of different times be marked on our chart as described above, and the points joined, the line joining them will be the actual path of the planet in respect to the earth, and it will be curved, somewhat like that traced in the foregoing figure, having a series of loops, which mark its stationary points and periods of retrogradation in longitude.

Now, if similar observations be made with respect to Venus, an analogous path, with less frequent loops, will be found.

Now these motions of Mercury and Venus in respect to the earth, and the positions which at stated times they occupy, are exactly the same as though they revolved round the sun, the one in $87^{d}\cdot969$, the other in $224^{d}\cdot700$, and that the sun revolved with them round the earth; and if the motions of the other, called superior, planets be observed, and their paths traced, the result will be the same. Thus the system of Tycho showed a profound acquaintance with the actual motions of the heavenly bodies, and a most careful and accurate comparison of them.

But it will at once strike the reader that these motions of the planets in respect to the earth, on the hypothesis that, revolving round the sun, they, together with the sun, revolve round the earth, are precisely the same as they would be on the opposite hypothesis, that, everything else remaining the same, the sun did not revolve round the earth, but the earth about the sun.

It is a known principle of motion, to which reference has before been made, that if there be any number of bodies moving in any way in respect to one another, and if to each there be communicated the same velocity, all the velocities thus communicated being parallel to one another, and in the same direction, then, whatever may be the *actual* motions of the bodies after these new velocities are communicated to them, their *relative* motions will be precisely the same as they were before; that is, after the same times they will be at precisely the same real and angular distances as they were before; and thus the relative positions of all the rest, as seen from any one of them, will appear precisely the same on both hypotheses. Hence, then, the sun revolving round the earth, and the planets round the sun in any way, if we suppose that to the sun, the earth, and the planets, thus revolving, there be communicated severally *the same* velocities in parallel directions, whatever these may be, the *relative* positions of the sun, earth, moon, and planets, will be precisely the same as before; they will be, after the same times, at the same actual distances from us and from one another, and in the same positions in the heavens, as before. Now, let us suppose the velocity thus communicated to them in common to be that with which the sun has been supposed to move, but in an *opposite* direction. The earth, which before stood still, will now move with the velocity which the sun before had; and the sun, having now communicated to it a motion equal to that with which it before moved, but in an opposite direction, will stand still,—whilst the planets moving in respect to the sun precisely as they did before, will now, as they did before, revolve continually round him, and in the same times. Thus, then, it appears that the sun will, by the motion thus communicated, be at rest; the earth will revolve round the sun with the velocity the sun had before; and the planets, too, will all revolve round the sun, now at rest, precisely as they did before round the sun in motion. *Thus, then,* whatever be the relative positions, motions, and appearances of the sun and planets as seen from the earth, on the supposition that the sun, with the planets, revolves round the earth at rest, the same will they be on the supposition that the earth and planets all revolve, each in its particular orbit, round the sun at rest. And conversely, if the earth and planets all do in reality revolve round the sun at rest, the appearances which they will present, in reference to each other, and as seen from the earth, will be precisely the same as though the earth were at rest, and the sun carried the planets with it in an annual revolution about the earth. Whatever are the appearances corresponding to the last hypothesis, the same are those corresponding to the first. Now we have *shown* the actual appearances of the heavens to correspond with the last hypothesis: they correspond, therefore, to the first,—that is, they are the same as they would be if the sun were at rest, and the earth and planets each revolved, in its particular orbit, round him. And these are the only two hypotheses which can possibly explain the appearances in question. Also, the hypothesis that the earth is at rest in the centre of the system has in a preceding paper been shown to be false. It follows, therefore, that the other hypothesis—that of the quiescence of the sun in the midst of our system of planets, and the regular revolution of our earth with the rest of the planets about it, as a common centre,—is the true hypothesis, and that it constitutes the real system of the universe.

Since the apparent motions of the planet in respect to the earth are those represented by the looped line in the figure on page 279, it is manifest that there are certain periods, when, being at any of these loops in its apparent orbit, its motion is in a contrary direction from that which is its *general* tendency. At those periods it is said to *retrograde*. Moreover, there are certain periods of its motion in each of these loops, when it is not moving at all in the general direction of its motion or the contrary, neither to the east nor the west, but directly towards or from the earth; although at these periods it rapidly alters its magnitude and brightness, by reason of the variation in its distance, yet it does not at all alter its position in the heavens: under these circumstances it is said to be at its *stationary* point.

The following are the arcs through which the planets severally retrograde:—

Mercury, from .	9° 22′ to	15° 44′
Venus	14° 35′ —	17° 12′
Mars	10° 6′ —	19° 35′
Jupiter	9° 51′ —	9° 59′
Saturn	6° 41′ —	6° 55′
Uranus	3° 36′	

THE SYNODIC REVOLUTIONS OF THE PLANETS.

THE synodic revolution of a planet, is the time of its returning from any position in respect to the earth into that position again. It is evident, that since the periodic time of the earth is different from that of each of the other planets,—these are perpetually altering their positions in respect to it, the one gaining perpetually in longitude upon the other. Moreover, it is evident that the same relative positions in longitude will be a second time attained, or they will have completed a synodic revolution, when one has thus gained a space equal to 360° upon the other, or some multiple of 360°; for whatever was the angular space which before separated them, it will now be 360° added to that space, or some multiple of 360° added to it,—that is, measured on the circle it will be the *same* separation.

Now, we may readily calculate under these circumstances what are their mean synodic motions. Let us take their mean daily motions in longitude, and subtract them from one another. We shall then have their relative daily motion, or the daily gain of one on the other in longitude. All that we want, then, is to know how many of these daily relative motions will make up 360° of relative motion, or of gain of one on the other in longitude; and this is done at once, by dividing 360° by this difference[*].

The sidereal and synodic times of the planets, or the times after which they return into the same position in respect to the earth, are as follows:—

	Sidereal time. Days.	Synodic time. Days.
Mercury	87·969	115·8774
Venus	224·7	583·9209
Earth	365·256	
Mars	686·979	779·9364
Jupiter	4332·584	398·8846
Saturn	10759·219	178·0919
Uranus	30686·820	369·6563

Not only can we, however, thus find the time after which any two planets will occupy the same relative positions, but we can extend this method of computation to three or four, or, indeed, to the whole number. It would be possible to tell when all the planets would occupy the same relative positions as at the present. It has, indeed, been calculated by Laplace, that the common synodic time of the six great planets is 17,000 millions of years. The manner of this computation will easily be understood from one case of its application.

Suppose it were required to find when the earth, Venus, and Jupiter, would occupy again the same relative positions as at the present moment. The synodic period of Venus and the earth is $583^{d}\cdot9$. We know, then, that after this period, or any complete number of times this period, Venus and the earth will be in the same positions relatively as now. Moreover, the synodic period of the earth and Jupiter is $4332^{d}\cdot5$. So

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[*] The following very simple algebraical formula determines the synodic time in terms of the periodic times. Let A and B be the periodic times of any two planets in days, then are $\frac{360}{A}$ and $\frac{360}{B}$ their mean daily motions, and $\frac{360}{A} - \frac{360}{B}$ is their relative daily motion. Let S be their synodic time in days, or the number of days in which they separate from one another in longitude through 360 $\therefore \frac{360}{S}$ is their daily separation or relative motion.

$$\therefore \frac{360}{} = \frac{360}{A} - \frac{360}{B} \therefore \frac{1}{S} = \frac{1}{A} - \frac{1}{B}, \text{ and } S = \frac{B - A}{AB}.$$

that Jupiter and the earth will be in the same relative positions as now after any complete number of times *this* period. If, then, we can find any two numbers such that the synodic time of Venus being multiplied by the one, shall equal the synodic time of Jupiter multiplied by the other, we know that after the number of days represented by either of these products, Venus will be in the same position in respect to the earth as it is now, and the earth will be in the same position in respect to Jupiter,—that is, all three will be in the same relative positions as at the present moment. This process is, in point of fact, that of finding a common multiple of the two numbers. Any such common multiple will give the time after which the phenomenon will return,—the least will give the *first* time. The calculation of these matters formed an important feature in the labours of astrologers, and many wonderful things were believed by them of their relation to the affairs of men and the destinies of empires.

Others of the planetary phenomena believed to be pregnant with important consequence in human transactions, were their conjunctions. When two planets have the same longitude, they are said to be in conjunction. The time of such a conjunction is readily found. Suppose the present longitude of two of the planets to be known; their difference will be the angular space which one will have to gain on the other before such a conjunction can take place. Now, subtracting their daily motions in longitude from each other, we know how much they gain daily. We can, therefore, at once tell how many days it will take to gain the whole existing difference of longitude, and thus bring the planets actually in conjunction. The same sort of calculation may be extended to the conjunction of any number of planets.

It is said that a conjunction of five planets took place, and was observed in China, in the year 2500, B.C. There was another of which we have authentic record A.D. 1186: it took place between the wheat-ear of the Virgin and α Libræ. Great disasters were predicted as the consequence of this conjunction, belied by the result.

It must, however, be admitted in favour of planetary influences, that at the moment when the cannon were announcing to the people of Paris the peace of 1801, the Moon, Jupiter, and Venus, were in conjunction somewhere near the heart of the Lion.

Laplace calculated the secular periods of Jupiter and Saturn from a conjunction of these planets observed by an Arab astronomer on the 30th of October, 1007.

Kepler remarked, that in 748 of Rome, Jupiter, Mars, and Saturn, were in Pisces in February and March, and that in April and May Venus and Mercury were in conjunction with the sun; and from these facts he concluded, on astrological principles, that our Saviour's birth took place in that year. Such was the weakness of one of the greatest men who have ever devoted themselves to science.

ON PROTECTIVE INK AND PAPER.

[Continued from p. 172.]

THE Commission applied to MM. Didot, and requested from their liberality a few experiments, and from their experience an opinion, on the value of this process. These gentlemen did not hesitate to declare that it would be vain to attempt the restoration of the design on paper so prepared, and afterwards bleached. They considered it very practicable to print a design, analogous to that previously described, in two inks at the same pull.

These gentlemen were also kind enough to make several experiments with us, in which were employed, for one part, the delible varnish-ink, and for the other, an indelible ink prepared by whitening common printing-ink with a considerable dose of sulphate of barytes. We lay before the Academy the results of these experiments, which, notwithstanding the imperfections of first attempts, have convinced us that this means might be used in practice with accuracy and economy, if the presses which it requires were obtained.

But after what we saw in the establishment of the MM. Didot, and from our own knowledge of typographical processes, we feel it a duty to add, that the real efficiency of this mode is confined to the prevention of the bleaching of the used stamp. If that of falsification in addition be contemplated, it can only be carried to the extent which the Board of Stamps require, by a better choice of design; and as every design typographically printed can always be reproduced by hand, this paper, though offering some difficulties to the falsifier, does not present to him any which are absolutely insurmountable. If the Board of Stamps would give up the hand-made paper, and adopt that made by the machine, they would find that cylinder-printing would give them the means of preventing the bleaching of the used stamp, and also—the falsification of public and private deeds.

The Commission does not shrink from the discussion of the respective merits of these two kinds of paper. If it should be decided to be desirable, they would institute experiments for the purpose of discriminating their characteristic qualities. At present, they say no more than that in advising processes which will not be perfect unless machine-made paper be employed, it is evident they consider this kind of paper capable of furnishing to the Government every guarantee which they are bound to require.

Hitherto, we have not noticed a case which often occurs in the various applications of stamped paper. It must be of a kind that will bear common printing, for there are many instruments of which large numbers are required, and that must be stamped, which, with the exception of some blanks left for filling up by hand, are previously printed upon this paper.

The stamp-paper ought, therefore, to possess the property of sustaining the damping which the typographical printer gives to the paper *before he makes use of it*. The Commission felt obliged to submit the paper which they recommend to this kind of proof, and they present specimens of it, so proved, to the Academy. They found no injury produced by the operation; the design was uninjured, and the impression perfect.

The bleaching of used stamps may therefore be prevented by the following methods:—

1. By printing, with a cylinder, on machine-made paper, in common ink thickened by plaster, designs of great delicacy.
2. By printing, with the copperplate press, either on machine- or on hand-made paper, in common ink thickened by plaster, designs of great delicacy obtained by a mechanical process.
3. In printing, with the typographical press, on hand-made paper, small figures in two parts, one delible, the other indelible. For the indelible ink, common printing-ink, whitened by sulphate of barytes, may be used; and for the delible ink, a mixture of hatters' sediment, chalk, and varnish.

But, as of these processes, the two first only can be used, if it be proposed to prevent at the same time the falsification of the written character, we do not offer the third but with the reservations we have previously made. We insert it rather to put in a strong light the inefficiency of the typographical process, than to recommend its adoption by the Board of Stamps.

SECTION III.—*Falsification of the Written Character in Public and Private Documents.*

WE have already stated that the Commission consider the question of the bleaching of used stamps, and that of falsification of the written character in public and private documents, to be inseparable.

Among the several modes of falsification, the most difficult to accomplish, and the easiest to prevent, are those in which a part only of the document is operated upon, and it is these which we now propose to consider.

In order to effect a partial falsification on paper covered with a delible design, it is necessary that the design be either preserved or reproduced. Both these operations require to be done by hand, and it is certain that they may be rendered impracticable even to the most skilful.

It is indispensable, as we have previously stated, to reject, in the composition of the design, all irregular figures, all personages, all ornaments, and to adopt geometrical figures, perpetually repeated, and, in order to ensure their perfect resemblance, executed by mechanical means.

The Commission, in its former report, advised the use of designs produced by the aid of the rose-engine. Such designs would certainly answer the end in view, but better may be obtained. Figures, of great regularity, of absolute identity, and of invariable form, are produced by the steel-roller process, a kind of engraving, by which, after an original of extreme perfection has been prepared, transfers may not only be made without the slightest injury to it, but the original may be still corrected and improved to an indefinite extent.

The Commission have examined with great interest some designs executed in this manner, which have been submitted to them by M. Emile Grimpé, one of our most able mechanicians.

They are a species of mail- or net-work, composed of small hexagons, which, seen by a lens, present very minute geometrical figures, perfectly identical, and of which the most able artists, notwithstanding our entreaties, have been unwilling to attempt either the reproduction, or the charging, by hand. M. Grimpé diversifies his designs; he can produce, by his processes, stars varying in the number of their rays; he can trace spirals, &c.; and thus give the power of changing the design of the stamp at will.

There is also an essential difference between the propositions of M. Grimpé and those of the former Commission. Your Commission having had in view the fraudulent bleaching of the old stamped paper only, suggested for its suppression the printing of a delible mark about an inch and a half long in the middle of the sheet; M. Grimpé, aiming at the prevention of all falsification of the written character, proposes to extend his design over the whole surface of the sheet of stamped paper.

Your Commission, perfectly convinced that the designs of M. Grimpé would attain the end they had in view, took great interest in ascertaining that they could be printed in delible ink. With the assistance of this skilful artist, they made the following essays:—

The design was engraved on a cylinder. Proofs were obtained from it, after it had been inked by hand with the delible varnish-ink, without difficulty.

We were willing to go further, and take proofs in an aqueous ink also; but, on attempting to ink the cylinder by hand, and then to take the proof, it was found that the ink dried too rapidly, and could not be transferred to the paper. When the employment of an aqueous ink is attempted, the inking and the printing should rapidly succeed each other, so that there should not be sufficient time for the ink to lose its fluidity before the pressure, which is to tranfer it to the paper, can be given. In short, not only was the cylinder lent to us by M. Grimpé, necessary, but also the printing-machine of which it forms a part.

M. Grimpé did not possess the kind of printing-press which we required; but, by his assistance, we found one that answered our purpose to a certain extent, at the establishment of M. Godefroy, at Surênes, and which had been used in the printing of linen, calico, &c. M. Godefroy was kind enough to place it at our disposal.

The cylinder engraven by M. Grimpé having been adjusted to this press, we printed several rolls of machine-made paper, and obtained impressions quite as clear as could be expected from a machine which had not been constructed for printing of paper.

We substituted for the machine-made paper long rolls of paper, made by glueing sheets end to end by hand, and were convinced that the printing of them was equally practicable, and the results satisfactory.

The ink which we employed was made with moulders' plaster, and writing-ink of the best quality* ground for a long time together. The pressmen considered it excellent for the purpose.

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* The writing-ink used was that labelled "*encre double de la petite vertu*," well known for its excellence.

If some fault should be found with the specimens we lay before the Academy, it must be recollected that we were under the necessity of printing upon paper with a machine which had been constructed for printing on cloth; slight as this inconvenience was, we had it not in our power to remove it.

The Commission would recommend, if designs were adopted similar to those which have been submitted to them by M. Grimpé, that the lines should be extremely delicate, and of little depth. Blacker ink, and more nearly resembling ordinary ink, might then be used; there would be less necessity to thicken it with plaster; and finally, the difficulty of imitation by hand would be increased.

It must be understood, that after an impression has been taken, the projection of the ink-line must be destroyed either by calendering or cylinder-pressing, in order to avoid the indentation or hollow which the sinking of the ink into the paper might produce.

The Commission, therefore, persist in asserting, that by printing a delible design on machine-made paper, in an aqueous ink, and by means of a cylinder engraven with extremely small, regular, and similar figures, a protective paper might be obtained which would be well adapted to prevent partial falsifications. But would it not also be possible to obtain a sufficient protection against this species of fraud, by the use of typographical processes and two kinds of ink?

This is difficult, if not impossible, although among the processes the knowledge of which we owe to M. Grimpé, there is one which might complete the process. In fact, the circumstance which renders the use of this mode unavailable against partial falsification, excellent as it is against the bleaching of the old stamps, is, that such designs as can be printed typographically are never sufficiently delicate, and therefore they can always be imitated by hand.

But after a design, the most delicate that can be obtained in two inks, has been printed, an embossment may be given to the whole surface of the paper by passing it between two finely-grooved cylinders. The difficulty of partial falsification is increased by this means; for the effect of the embossing is to obstruct the pen of the person who would restore the obliterated design: he could not introduce it into the furrows, and his attempt, examined by a lens, would exhibit the fraud.

We mention the embossment, in order that we may omit nothing which can enlighten the Board; but the Commission, after mature examination of the process, feel obliged to reject it. They are convinced that the embossment greatly diminishes the strength of the paper in the direction of the furrows which it produces. The weakening effect is evident on the slightest experiment. This is a very serious objection, particularly when we consider it with regard to paper intended to preserve important documents during a long series of years, liable to be exposed to numerous accidents of friction and of carriage, and to be crumpled and folded in every variety of manner. Further, paper so embossed would present great inconvenience to writers, the pen, springing from one ridge to another, would defy all command in the direction of it, and the written characters would be deficient in firmness; rapidity and freedom would therefore be greatly diminished.

The latter annoyances would not take place if, as M. Grimpé further proposes, the embossment should be deferred until after the writing is completed, in the manner of an extra stamp to ensure it against attempts at falsification.

But then it becomes indispensable to examine whether this embossment could not be removed by the employ of certain chemical agents which would cause the paper to swell, or by some mechanical means which would smoothen the surface. The embossed papers of M. Grimpé were not found to be proof against such means; we smoothened them perfectly, and removed every trace of the embossment. We therefore rejected this process; for if it be applied after the writing, it is of no use; and if applied before it, it interferes with the action of the pen, and throws new difficulties in the way of the skilful penman.

The Commission would willingly adopt one application of embossment, to which M. Grimpé was led by their requisitions. Desiring to have from him an indelible stamp which would exactly coincide with the delible vignette, M. Grimpé printed a design in delible ink, and on the margin for the whole height of the sheet he continued the design, uninterruptedly, without ink, in the manner of embossing, or colourless stamping.

It would certainly be shorter to remake stamped paper, than to use again the sheets of such a paper after bleaching.

Thus, with a delible design, inimitable by hand, and impossible to be transferred to the stone, we have, definitively, everything necessary to prevent the bleaching of stamped paper, and the perpetration of the fraud which we have termed partial falsification. But this species of fraud is not the only one to be feared.

We now come, in fact, to falsifications which are the most easy of execution, and consequently the most difficult to prevent. They are those in which some few words only of a document are required to be preserved, and all the rest to be removed, for the purpose of substituting others; they are those in which there is no necessity to preserve the whole of the paper, and in which, for example, the upper or middle part, on which are the stamps, may be removed, and the lower part only preserved, as on this is the signature accompanied by some few words which the falsifier may wish to employ.

This species of fraud can only be prevented by giving to the paper an indelible characteristic, and so completely distributed over its surface, that on an examination of the smallest fragment, the particular kind of stamp might be recognised.

Such is a characteristic of the paper printed typographically with two inks, already mentioned. In fact, it is impossible to obliterate the writing, and destroy the characteristic of such paper entirely, for the lines which are in indelible ink, upon it, permanently resist all attempts to remove them.

But printing in two inks can only be used with designs susceptible of imitation by hand; they offer no advantage whatever, when a falsification is contemplated to which time and skill can be devoted.

So that all the protective papers at present known are incapable of preventing a total falsification; and to such an extent are total falsifications now to be feared from the advanced state of chemical science.

The design of the Board, and those of M. Grimpé, present no greater obstacle to total falsification than common white paper.

The best security against total falsification known up to the present moment, is the indelible ink of the Academy. With this, every paper is secure; without it, as has been remarked, all are defective.

It cannot, therefore, be too frequently repeated, that all public Boards would be protected from such frauds, if they would make use of this ink; and the same may be said of all individuals who might adopt it.

But the Commission does not conceal that this change, apparently so slight, is, in reality, extremely difficult, and if proof were wanting, it would be found in the oblivion into which their recipe seems to have fallen since the period of their first Report; and, notwithstanding its simplicity, in the repugnance which ink-manufacturers seem to feel to its preparation.

Six years ago every one appeared to interest himself in the inquiries of the Commission; every one was anxious as to their result. Ministers, notaries, bankers, all the world, demanded insurance against falsification in writing. From the moment the recipe of the indelible ink was published, up to the present hour, no person has made use of it. A new habit was certainly necessary to be formed; persons must have prepared the ink themselves, or obtained the particular one in question; these, undoubtedly, were no great difficulties, but they were found too great, when the danger to be avoided was very uncertain and very distant.

Therefore, though they still strongly recommend the use of the acidulated Indian ink, the Commission have been led to point out a means of preparing a protective paper, the most effective possible for the prevention of total falsification.

This means is so easily deduced from the principles already laid down, that it is sufficient merely to refer to them. If, in fact, upon an endless sheet prepared with an indelible and very fine pattern, a very delicate delible design, inimitable by hand, be printed upon each of its surfaces, a paper would be produced which would render all falsification, both partial and total, perfectly impossible.

For the writing might be entirely obliterated, or certain words reserved and the rest effaced, but the delible design would disappear with the writing; and if a denial of the falsification of a document was attempted, the presence of the indelible pattern would always remain as evidence of the contrary.

There is nothing to prevent the substitution of an impression in unctuous ink upon paper already made, for the indelible pattern.

This has been very nearly realized by M. Coulier in a process by which he prepares a protective paper intended for commercial purposes. He prints an indelible border on the left, and he repeats the same border on the right in a delible ink. Paper so prepared appears to be superior to any that has hitherto been proposed, but it does not perfectly satisfy all the conditions proposed by the Commission. These require that both the delible and the indelible design should be distributed uniformly over the whole surface of the paper, and that the transfer of them should be impossible.

Notwithstanding the dislike which the Commission entertains to protective paper, they feel it, however, a duty to state a means by which an article of this kind, that would leave little to be desired, can be prepared: it would satisfy the three following fundamental conditions:—

1. It would preserve the evidence of its nature by its indelible pattern, so long as it existed as paper.
2. Its delible design would disappear under the influence of the agents which would affect the written characters upon it, and would change only as these were changed.
3. Its delible design could not be restored, either by hand or by transfer.

After having thus pointed out the preparation of a protective paper adapted to expose all attempts at falsification, the Commission hope that some attention may be granted to the following considerations:—

1. This protective paper, though superior to all others, still does not prevent the destruction of the writing, either by the falling, accidentally, of an acid upon the paper, or the same being done by design, excusable, on being attributed to chance.

The protective ink cannot be effaced.

2. This protective paper permits attempts at falsification. There are persons to be found who would be foolish or mad enough to discharge the writing and the delible design, and then attempt to restore the latter; it is true that, betrayed by their own act, they would be discovered and punished.

But the protective ink goes further than this; it prevents the crime, for it renders vain every attempt at falsification.

Thus, the surest means of preventing falsification of every kind, whether easy or difficult, is, decidedly, the employment of a protective ink. Its use ought to be made obligatory in all important documents, and in those that are liable to frequent attempts at falsification, such as state-papers, bank and other notes intended to be invariable, &c.

But as it might be considered going too far, to desire the extension of this obligation to individuals, or even to notaries or attorneys, it might still be useful and advantageous to furnish for their use a good protective paper, prepared according to the principles which have been previously laid down.

Besides, in the degree that the sale of stamped paper constitutes an important source of revenue to the state, is it necessary to guarantee it against fraudulent bleaching; and for this particular purpose, paper with delible designs might continue to be useful, even when the use of an indelible ink should have become almost universal.

If the discussion which the Commission has thus entered into, has been thoroughly understood, every one may judge of the use he can make of the means which have been indicated, and these we proceed to recapitulate.

By means of the indelible ink, not only is all alteration of the written character prevented, but the latter is rendered ineffaceable*.

By means of a delible design, all partial falsification, &c., to which certain documents are exposed, are prevented; but it does not prevent total falsification, nor the destruction of the written character.

By means of a delible design, combined with an indelible pattern, falsification of every kind, partial and total, is prevented; but the combination does not prevent the destruction of the written character.

The best protective paper is not, therefore, of equal value with a perfectly indelible ink; but we are convinced that a good protective paper may be of service to public offices, and to commercial purposes; and so much so, that we should not be surprised to see the consumption of stamped paper increase, if, to the legal conditions which enforce its use, there should be added, a sure protection to those who may use it.

The principles laid down in this Report will, therefore, lead to the following conclusions, which we have the honour of submitting for the approval of the Academy.

1. With regard to the paper prepared by the *Administration de l'Enregistrement et des Domaines*, and upon which the Minister of Finance has consulted the Academy, the Commission are of opinion, that this paper will not prevent the bleaching of the used stamps, nor the falsification of public and private documents.
2. If the Board are desirous of continuing the use of typographical printing, the Commissioners are unable to point out any process which will prevent falsification of the written character; but they are of opinion that, by means of a design printed in two inks, the one delible, the other indelible, the fraudulent bleaching of the used stamps would be counteracted.
3. But if the Board would adopt printing by cylinders, or by plates, they would accomplish the prevention of every kind of falsification, and at the same time stop the bleaching of used stamps. It

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* Persons who have made some progress in chemical studies, and who know that Indian ink consists of carbon most minutely divided, will understand, without particular explanation, the motives which decided the former Commission in the selection of this ink. They will, in fact, recollect that carbon is insoluble, and that it resists the action of all known agents at low temperatures, and that before it would be affected, the paper would be destroyed.

But if Indian ink is always indelible, so far as chemical agents are concerned, it is quite possible to efface it by mechanical means; and this might be done in all cases in which it had not penetrated into the substance of the paper.

There is, therefore, a certain proportion to be ascertained between the acid, or the alkali, which, added to the Indian ink, is intended to produce this penetration, and—the sizing of the paper itself.

The more the paper is sized, the more of the acid, or of the alkali, is required to effect the penetration, and this, alone, constitutes the guarantee against falsification. The proportions given by the Commission are intended for the common papers of commerce; for peculiar papers, and such as are unusually sized, these proportions must be increased.

If the paper written upon be a little damp, the penetration, and the amount of security given by it, are increased: therefore, in important cases, it would be advisable slightly to damp the paper, and after waiting a minute or two, to permit the humidity to penetrate quite through the sheet, to write with Indian ink recently dissolved in the acidulated water.—(*Note by M. Dumas, the Reporter*).

would only be necessary to make a paper prepared with a very delicate indelible pattern spread over every part of the surface. Upon both faces of this paper should then be printed a delible design, composed of very small geometrical figures, perfectly similar, and inimitable by hand. Such a paper would be strongly recommended by the Commission; it would satisfy the wishes of the Board, and be more than is required by individuals, inasmuch as, though it would not prevent the destruction of the written character, it would counteract all falsification.

4. Finally, the Commission, recollecting that the best preservative against all falsification of the written character consists in the use of acidulated Indian ink, are of opinion, that the Board would do well to render the use of this ink obligatory upon persons employed by them, in all instances when desirable that the writing should remain proof against all attack.

INKS.

Delible Inks, not unctuous, for cylinder-printing.

No. 1.

Common ink, properly thickened by evaporation in the water-bath.

No. 2.

Common ink, properly thickened by sulphate of lime, and ground with it for a long time.

These delible inks have the advantageous property of resisting the action of water, so that paper printed with them may be damped, and subjected to the operation of the typographical and lithographical presses, without injury to the design.

Delible unctuous, or delible varnish-Inks, for printing designs typographically in two inks.

No. 1.

Chalk, washed and dried	24 parts
Ink-sediment, dried	3 „
Ultramarine	2 „
Varnish	*in sufficient quantity.*

No. 2.

Chalk, washed and dried	24 „
Ink-sediment, dried	1·5 „
Ultramarine	1 „
Varnish	*in sufficient quantity.*

VARNISH.

Linseed oil	60 „
Gallipot[*]	150 „

The mixture is to be heated, and when entirely melted, to be strained through a fine cloth.

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[*] Burgundy pitch.

INDELIBLE INKS.

No. 1.—*For writing with quill-pens.*

Indian ink, dissolved in water acidulated with hydrochloric[*] acid of commerce, and marking 1½° on Beaumé's areometer[†].

No. 2.—*For writing with metallic pens.*

Indian ink, dissolved in water rendered alkaline by caustic soda, and marking 1° on Beaumé's areometer.

No. 3.—*For printing the indelible designs and patterns.*

Common printing-ink, whitened with a sufficient quantity of sulphate of barytes, either artificial or natural, which has been ground in water for a considerable time.

Report presented to the Academy, 13th February, 1837.

In 1831, when the Commission presented their first Report on protective ink and paper, scarcely any other than animal size was used in paper-making. An Indian ink solution, slightly acidulated, was, therefore, sufficient for the penetration of the paper of that day. Since that time, however, size, prepared from starch, resinous soap, and alum, has been brought into almost general use; and such acidulated Indian ink does not operate upon paper made with it so favourably; it, therefore, becomes necessary, either to increase the proportion of the acid, or to use the alkaline solution only, as given in the Report of the present year.—*Note attached to a Report on Protective Paper, invented by M. Mozard, presented (by a Commission formed of the same Members) to the Academy, 13th February, 1837.*

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[*] Muriatic acid, about sp. gr. 1·28, so called by the French chemists.—Edit.

[†] As Beaumé's areometer is but little used in this country, it may be useful to state that if water be taken at 1000 sp. gr.
1° on this instrument is 1007 ,,
1½° . ditto . 1010 ,,
2° . ditto . 1014 ,,

For the acidulated solution.—To a pint of water add about three teaspoonfuls of muriatic acid.

For the alkalized solution.—To the same quantity of water add 100 grains of pure caustic soda, of the kind prepared for chemical purposes.

About 40 grains of Indian ink may be dissolved in a pint of a solution. This will make an ink sufficiently black for ordinary writing, but it is evident it may be very easily made darker or lighter at pleasure.

The Indian ink should be of the best quality. Care should be taken that it be uniformly good throughout the stick.

An inferior kind, hard, gritty, and possessed of little colouring power, is sometimes imposed upon the purchaser, by the deceptive practice of attaching a little bit of good ink at each end of the stick. A good mode of examination is to break the stick in two: this defeats the above trick, and exposes the fracture; that of good ink is brilliant, angular, and clean; the bad is dull and earthy.—Edit.

A NEW FRENCH LAMP.

An evening meeting of scientific and practical men took place in the lecture-room of the Gallery of Practical Science, in Adelaide Street, on the 3rd of April, to hear an explanation by M. Duverger, of Paris, of a new lamp, invented by him, in which the use of any mechanism for the purpose of either supplying the wick with oil, or of raising the wick as it is burnt, in order to remove the carbonized portion, is superseded by the ingenious and novel application of an hydrodynamical principle. As M. Duverger delivered his discourse in his native tongue, in a low tone of voice, and with too much rapidity of utterance to be intelligible to three-fourths of his hearers*, we think an account of his lamp may be interesting to our readers, since it has excited a good deal of attention, and been productive of much difference of opinion as to its merits or defects, both of which are, of course, overrated by the respective partisans or opponents of the invention.

We must premise that the lamp that has as yet been exhibited by M. Duverger is constructed for the purpose of illustrating the *principle*, and is by no means of the form intended for use and sale; but as the merits and defects are involved in the principle, and not in the application, this will not prevent our readers from forming an opinion from our account; and having enjoyed the advantage of several private conversations with the inventor, we can point out in what the distinction between the *model* and the real lamp will consist.

From a hollow stand of any form, the capacity of which is determined by considerations to be hereafter explained, arises a cylindrical tube, terminating at top in a reservoir, to contain the quantity of oil with which the lamp can be charged. At the bottom of the tube, and within the stand, is an inverted conical cap, having a minute aperture in its side, which forms a communication between the tube and stand. The oil being poured in at the top of the tube, fills both this and the reservoir before any quantity can escape through the aperture into the stand,—this, consequently, remains nearly full of air, slightly condensed by the pressure of the superincumbent oil, and it is clear that as the oil drops through the aperture into the stand, a corresponding drop of air must escape and rise in the tube.

A second tube of smaller diameter is then inserted into the first,—the upper end of the inner tube terminates in an argand burner, to which a glass chimney is applied, as usual, the cylindrical space for the wick communicating with the tube, and the wick is of sufficient length to stand up an inch or more above the edge of the burner, no means of adjusting the height of the wick being required, as will be presently seen. The bottom of the inner tube is expanded into a trumpet-mouth, *nearly* to fill the diameter of the larger one.

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* We may observe, in a note, that the room on this occasion was lighted by the oxy-hydrogen light, this being the first *time that* that resplendent means of illumination has been used for such a purpose. *The effect* was most gratifying to the *audience*, but as M. Duverger was exposed to the full glare of the light in his eyes during his lecture, it must have been distressing to him, and may partly account for his hurried manner.

It is obvious that when the second tube is first gently inserted, the oil rises freely in it, till it stands at the same level with that in the reservoir. But as the bubbles of air, which are constantly and equably rising from the hollow stand, ascend in the inner tube only, on account of its funnel-shaped orifice protecting the outer one, the column of fluid in that inner tube becomes specifically lighter than that in the outer, as consisting of oil and air mingled, instead of oil alone. The consequence is, that the mixed fluid in the inner tube will rise above the level of that in the outer, till the weight of this elevated portion counterbalances the difference in the specific gravity of the columns.

The lengths of the two tubes are so adjusted as to admit of the oil flowing over the edge of the burner. After saturating the wick, thereby preventing its charring downwards to the burner, the superabundant oil flows over into the reservoir, and a continued circulation between the two tubes is maintained, till the oil, by percolating through the aperture, has filled the stand, displacing the atmospheric air contained in it, and thus stopping the action of the lamp.

The lamp shown by M. Duverger contains, when charged, about three-fourths of a pint of oil, and will continue, with that quantity, to support a steady clear light from a burner of middling diameter for seven or eight hours. To replenish the lamp, four methods are proposed by the inventor,—either, having withdrawn the inner tube, to unscrew the outer one from the stand, and to pour the oil back into the oil-can, from which the reservoir may be again refilled, the tube is then replaced, and the inner one again inserted within it. When this mode is adopted, however, care must be taken previously to empty out what oil may remain in the outer tube before this is unscrewed.

The second and more preferable mode of emptying the stand is by a stop-cock, adjusted to it. The third is by injecting air through a tube into the stand, and thus forcing the oil to re-ascend in the tube; and, lastly, M. Duverger proposes using the inner tube as a pump, to raise the oil again into the reservoir. But these two methods are obviously so objectionable as to be practically useless. Those who know the trouble of trimming and cleaning any lamp will see the objections to the others; but as none but an idle footman ever dreamed of a lamp that was perfectly free from all trouble or dirt as long as animal, or indeed any *oil*, is used, the question is not, whether M. Duverger's is free from all objections, but whether those which inevitably attend it are not counterbalanced by its advantages.

These are, as we have shown, the absence of any merely mechanical adjustments; no racks and pinions or wormed cylinders for raising the wick, no fountain-reservoirs with valve to regulate the supply of oil. And secondly, as the oil is entirely *below* the light, there is no shadow to be thrown by the vessel containing it, as must inevitably be the case with all those lamps in which the oil *descends* to supply the burner.

The quantity of oil, the time the lamp is to continue burning, the intensity of the light, are all matters to be governed at pleasure, by varying the size of the reservoir, stand, and burner; but our ordinary lamp-makers, though they may produce a brighter light than that of M. Duverger's lamp, do so by a prodigal and most unscientific waste of material, owing to the total neglect of any sound principle in the construction of their chimneys and of their burners. In this respect they will do well to take a lesson from the sound scientific views on this subject entertained by M. Duverger, who shows that he is in every way a worthy élève of the Ecole Polytechnique, and of M. Monge, to whom he is indebted for his mathematical and mechanical acquirements. With respect to economical considerations, the merits of the new lamp have yet to be tested by a series of accurate and recorded experiments; but, as far as we can judge, they would prove in every way favourable to M. Duverger's invention.

Many of our readers are probably not aware that a purer and stronger light is obtained from vegetable oils, such as the cocoa-nut and palm-oils, than from the animal oils. Cochran's lamp was contrived to burn the former, a copper disk supported over the chimney imparting sufficient heat along a wire leading into the reservoir to keep the mass liquefied when it has been previously sufficiently warmed before the lamp is lighted. Another of our most skilful and scientific lamp-makers* is about to produce a cheap and simple lamp for the same purpose, which we can assert, from personal inspection, will place this hitherto-neglected material, with all its advantages, as regards economy and efficiency, within the reach of all.

From the foregoing account of the *principle* of M. Duverger's lamp, it is clear that by increasing the relative quantity of air supplied to the inner tube, the fluid may be raised to a greater height above the level of that in the reservoir, and, *strictly speaking*, there is no *assignable* limit to this difference. We regret that M. Duverger should be so far misled as to the practical application of this extension of his principle, as seriously to pronounce the lamp but a secondary object, and to propose the adoption of that principle to raising water in wells. However lightly he may prize the mathematical or the pure-scientific attainments of English engineers and others, we can assure him that they are far too well versed in practical hydrodynamics not to see the fallacy of his reasoning on this part of the subject. And we would also counsel him not to descant on his discovery of a *new* hydrodynamical principle, for the only novelty to which he can justly lay claim is that of applying the principle to a lamp, and with the merit of that he may well rest contented.

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* Mr. Parker, of Argyle Place.

SECTIO-PLANOGRAPHY.

We believe we may say without fear of contradiction, and certainly with no intention of giving offence, that comparatively a very small number of persons of landed property understand a *plan*. This is principally owing to that culpable deficiency in practical education, which at present exists in all the great establishments in which our more elevated ranks spend the most precious years of their lives. If but a small number understand thoroughly a plan, still smaller is the number who can comprehend a *section*, and that combination of both *plan and section*, which is necessary to be made and understood before a clear idea can be obtained of the position of a particular part or point, with regard to all the other parts or points that surround it in every direction. The plans and sections of a single edifice are not to be thoroughly and satisfactorily understood, even by the architect, at a glance; lines in one, shrink into points in the other; planes become lines; and he cannot prudently judge of the whole, until, by sufficient study, his imagination so distinctly embodies the one, that he can instantly and involuntarily combine it, when looking at the other.

If this be true of delineations of objects so limited in magnitude as even the largest mansions, and whose delineated areas rarely extend to the borders of a sheet of double-elephant, how far greater must be the mental effort, when designs run from sea to sea, over a country of varying altitude and depression, and whose delineations, even when miles are compressed into inches, defy the continuous longitude of an "endless sheet." Persons who have never visited that Office of the House of Commons where the plans and sections of intended rail-roads are annually deposited, can have no idea of the scene. One would think that the whole country had been stripped of its epidermis, that it had been manufactured into striated paper, and deposited there.

When a *line* is to be examined in a committee of the House of Commons, it is soon found that it is not a mathematical one in any sense of the word. The *breadth* of the line, and its horizontal vagaries, generally requires the broadest kind of paper, and sheet after sheet, or rather ream after ream, until the scale of length prescribed by the "Standing Order" is accomplished. The *depth* of the line, and its vertical undulations, are far more reasonable in their demand for breadth, yet they have the same insatiable appetite for length. When the plans and sections necessary for the inquiry intended first appear before a committee, they have no very alarming appearance;—a portfolio, of no very gigantic dimensions considering the occasion, generally labelled "PLAN," in gold upon red, and a cylinder, perhaps a foot high, and of a diameter varying from two inches to eight, embodies the SECTION; but when under the process of examination, cross-examination, re-examination, and questions by committee, the engineer to the undertaking and his assistants, and the opposing engineers and their assistants, have turned over and turned back, unrolled and rolled, and unrolled again, portfolio and cylinder, with the most contrary intentions of comparing and combining, and proving and disproving, and have covered tables and floor with their convolutions, some little idea may be formed of the quantum of accurate information which an impartial and constantly-attending member of a committee may obtain after fifty days' inquiry, particularly if he happen to be a "gentleman born."

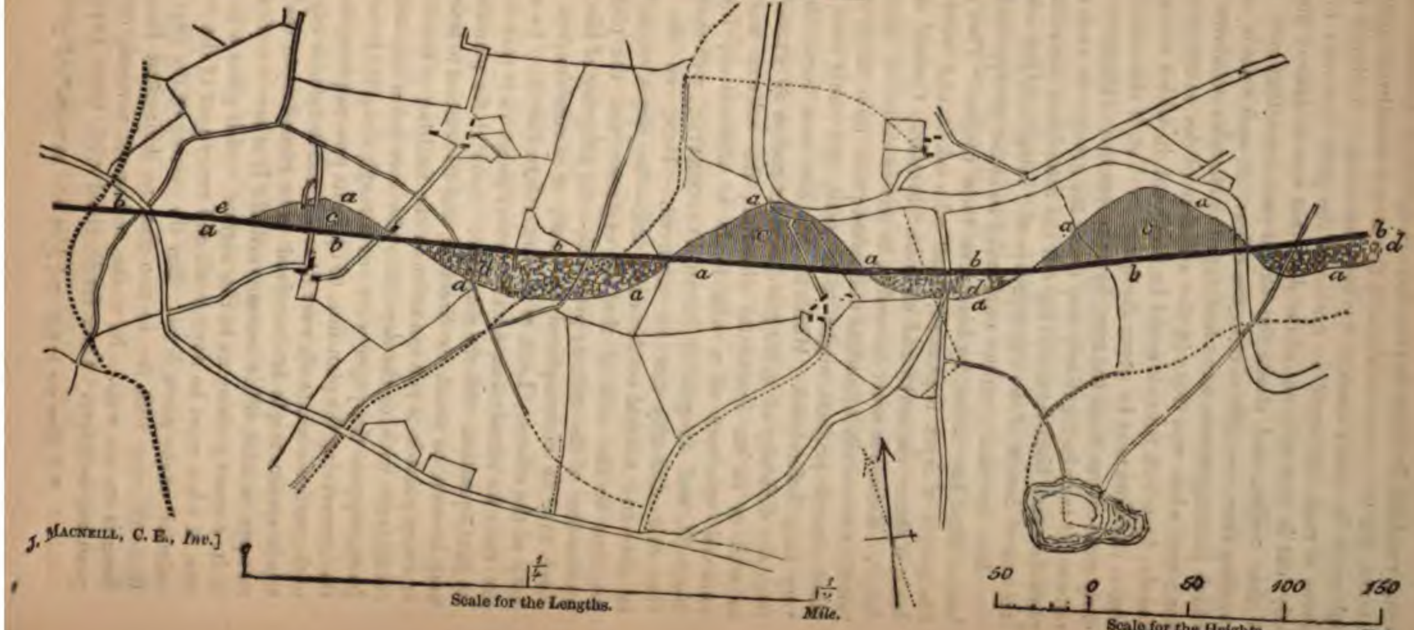
But, as every country gentleman is not a member of a parliamentary committee, it may be hastily presumed that these perplexing mysteries can never annoy him. With the country scored as it is with intended railways, no such gentleman can escape. This very portfolio and cylinder, or some few yards of each, is certain to roll into his hall, and be deposited on his library-table, either by friend or enemy, and he will find, sooner or later, that, though in undisputed succession of an ancestral estate, rich in preserves where poacher never entered, though a lover of that nature which has spread some of her loveliest scenes within his domain, and possessing health, and a keen relish for the field, this mysterious pair of unlike forms are the certain precursors of mighty evil. After a little time of execration on the COMPANY and their agents, he sits down with his attorney and surveyor. The three together can decide, within a mile, how near the railroad will approach that wood, or this lawn, and, perhaps, the amount of the lop-sided angle it will fill up of that sheet of water, which cost his grandfather thousands to create, in the geometrical style of gardening of his day. But the question, how the railroad is to maintain its level, and run down the side of that valley, and over that ridge, strikes out numerous inquiries, which end in the unrolling of the cylinder, and, in fact, nothing more; for after hours spent in attempts to combine the Section with the Plan; to connect the horizontal conditions of the one with the vertical conditions of the other divorced as they are, the consultation generally ends, with, perhaps, a point or two accurately ascertained, but assuredly with a vexatious conviction, that some great mischief is about to be perpetrated, but in what way, or to what extent no clear notion has been obtained. What is the consequence? The landed proprietor either opposes the bill, shutting his ears against every proposition which might mitigate or remove the evil, and putting himself and the promoters to immense expense; or, as the final event is, nine times out of ten, the same, he saves great pain, cost, and vexation, by doggedly submitting to his executioners. His estate is then dismembered, and his enjoyments destroyed, in a legal manner, under the humane superintendence of the acting engineer of the company. In two or three years, if the calls are paid up, instead of the green, sheltered, turfed and meandering lanes, there will be the sterile, exposed, iron road, having the very picturesque qualification of "no curve of less radius than a mile." There, where the owner used to meet the lamb and its mother, and hear the tinkling bell of some fellow-wanderer, he may be crushed by a locomotive; for though he hears its snorting a mile off, he has but a second or two to climb up the "cutting of one to one," to save his life. Game he may find at his poulterer's in Jermyn Street, but there is not a wing in his closest preserve.

Mr. Macneill, by the invention we are about to notice, has made some small atonement for the terror he spread through the rural population a year or two ago, by his tables for facilitating "cuttings and embankments," and for the attacks his profession has hitherto covertly made upon the

SECTIO-PLANOGRAPHY.

SPECIMEN OF ITS APPLICATION, IN THE DELINEATION OF A SURVEY FOR A RAILROAD.

a Natural surface of the ground.
b The proposed Railroad.
c Its passage through elevated ground; a case of "*Cutting.*"
d Its passage across depressed ground; a case of "*Embankment.*"
e Its passage along level ground.



J. MACNEILL, C.E., *Inv.*]

country and the dwellers therein, and against which there was little means of defence, for the extent of the evil threatened was always matter of doubt, and could be easily masked by bold assertion. A simple line running across a map, conveyed no notion of the gash that was to be so fearfully cut in that hill, nor of the dam that was to destroy the perspective of that valley, and choke the old acquaintance that once ran free and gurgling from one side of it to the other. It spoke not of viaducts from which passengers can now look into the chamber-windows of his mansion; and what was there in the solution of its continuity, those dots merely, by which tunnels could be predicted, in which nitrogen will ever linger, and darkness be never dissipated? Yet, if this simple line be not early washed out from the map, by a process more expensive than a king's ransom, it may be legalized, may curse the ground on each side within a parliamentary boundary, driving out the astonished possessor, and teaching a fatal lesson of the consequence of "being troubled with a *line*."

But the enemy cannot now make so secret an approach; he can no longer blind his victims by his worse than useless "Plan and Section." The prayer of Ajax is granted, to all who ask it, and would to all who don't, if those who make and unmake "Standing Orders" would do their duty*. Railroads must assuredly, in certain cases, be executed: but the mischief necssarily attendant and consequent upon these numerous, and often gigantic projects, ought to be seen, and easily seen, by every eye, very long beforehand. Hitherto this has been impossible, principally from the difficulty of getting at a correct notion of where, in a vertical as well as in an horizontal direction, the railway was to go. The annexed specimen of a new mode of delineation, invented by Mr. Macneill, and designated SECTIO-PLANOGRAPHY, will show, at a glance, that this difficulty can be removed. Here, on the same surface, in close and natural combination, are,—the virgin surface of the earth previous to the visit of the fell engineer, and the plan-line and the section-line of the railway proposed,—the offspring of his unholy contact. Each may be contemplated by itself, or in combination with either or both of the others. Look at the natural surface-line, (*a a*,) and you can ascertain its correctness, &c.; for every point in it may be recognised by its juxta-position with the plan-line. Look at the plan-line, (*b b*); there may be seen, as usual, its direction, and its relation to lateral objects; but at every point in it you can estimate the facilities or the difficulties, by the coincidence with, or the departure above or below the surface-line, (*a a*). Now, suppose the paper were cut through along the latter line, (*a a*), and that, preserving the plan-line, (*b b*), in its present plane, the superior edges of the surface-line, (*a a*), were elevated, and its inferior edges depressed, the whole section might be conceived to be turned upon the plan-line, (*b b*), as upon an axis, till it became vertical; it is now a section of the country, in its correct position as to the surface, and it gives a perfect representation of what must be done at all and every part of the line, to obtain the railroad at the given level. But the cutting of the paper is unnecessary. Raise the map with the section-plan so delineated from the table, and hang it on the wall. Now the section is vertical, and in its natural position, as before. Once familiarized with these experiments, neither is any longer necessary. A *coup-d'œil* of a sectio-plan laid down upon a map is no longer a limited and merely superficial view as in former years. It shows, previous to a great and important operation being performed, the wounds and the tumefactions which must be produced by the operator, however skilful, and if the party whose estate is to be operated upon is still "recusant," he can ascertain if it be worth while to cut his own throat, or that of the engineer, before the professional "cuttings" of the latter scare away the mountain-nymphs of his home.

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* "Give me to see, and Ajax asks no more."—Pope's *Homer*.

SIMULTANEOUS METEOROLOGY.

In some "Instructions for making and registering Meteorological Observations," circulated by the South African Literary and Philosophical Institution, established at the Cape of Good Hope, and drawn up, it is believed, by Sir John Herschel, there is, among others, the following recommendation:—

"With a view to the better determining the laws of the diurnal changes taking place in the atmosphere, and to the obtaining a knowledge of the correspondence of its movements and affections over great regions of the earth's surface, or even over the whole globe, four days in each year should henceforward be especially set apart by meteorologists in every part of the world, and devoted to a most scrupulous and accurate registry of the state of the barometer and thermometer; the direction and force of the wind; the quantity, character, distribution of clouds; and every other particular of weather, throughout the whole twenty-four hours of those days, and the adjoining six hours of the days preceding and following*. The days recommended for these observations are, the 21st of March, the 21st of June, the 21st of September, and the 21st of December, being those or immediately adjoining to those of the equinoxes and solstices, in which the solar influence is either stationary, or in a state of most rapid variation. *But should any one of those* 21*st days fall on Sunday, then it will be understood that the observations are to be deferred till the next day, the* 22*nd.* The observation at each station should commence at 6 o'clock A.M. of the appointed days, and terminate at 6 o'clock P.M. of the days following, according to the usual reckoning of time at the place. During this interval, the barometer and thermometer should be read off, and registered hourly, and the precise hour *and minute* of each reading should be especially noted.

"For obvious reasons, however, the commencement of every hour should, if practicable, be chosen, and every such series of observations should be accompanied by a notice of the means used to obtain the time, and, when practicable, by some observation of an astronomical nature, by which the time can be independently ascertained within a minute or two*. As there is scarcely any class of observations by which meteorology can be more extensively and essentially promoted, it is hoped that not only at every station of importance in this colony but over the whole world, and on board ships in every part of the ocean, individuals will be found to co-operate in this inquiry. Every communication of such observations, addressed by channels as secure and as little expensive as possible, to the Secretary of this Institution, will be considered as highly valuable."

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* This is necessary by reason of the want of coincidence of *the day* in different parts of the globe, arising from difference of longitude. In order to obtain a complete correspondence of observation for twenty-four successive hours over the whole globe, it must be taken into account that opposite longitudes differ twelve hours in their reckoning of time. By the arrangement in the text, the whole of the *astronomical day* (from noon to noon) is embraced in each series, and no observer is required to watch two nights in succession.

In cheerful compliance with the above valuable recommendation, the observer to whom is confided the formation of our Meteorological Table, undertook the proposed series of observations during the period which has lately passed. The results will be found in the Table which follows, and which we transmit to the Secretary of the Institution at the Cape, as our contribution to the collection which is there proposed to be amassed and concentrated from the whole surface of the globe.

Observations of the same nature, and at the same times, have been taken at the Royal Observatory, Greenwich; at the apartments of the Royal Society, Somerset House; at the Observatory of Cambridge, &c.

As one of the most important ends of meteorological observations is their comparison with each other, and as, for this purpose, it is very desirable that observers should understand the use and adjustment of the instruments in their possession, we have been induced to select from these Instructions such of them as are of general application.

I.—*General Recommendations and Precautions.*

"1. The continuity of observations ought to be interrupted as little as possible by changes in the adjustments of instruments—in their places—exposure—mode of fixing—or of reading off and registering them. Whenever any alteration in these or any other particulars takes place, especially such as are likely to affect the zero points, or otherwise to influence the mean results, it should be noticed in the register.

"2. So far as possible, registers should be complete—but if by unavoidable circumstances of absence, or from other causes, blanks occur, no attempt to fill them up by general recollection, or by the apparent course of the numbers before and after, should ever be made.

"3. The observations should, if possible, all be made by one person—but as this may often be impracticable, the principal observer should take care to instruct one or more of his family how to do it, and should satisfy himself by many trials that they observe alike.

"4. The entries in the register should be made at the time of observation, and the numbers entered should be those actually read off on the respective scales of each instrument, on no account applying to them previous to entry *any sort of correction, as, for instance, for zero, for temperature, capillarity, &c.* All these and the like corrections, being matter of calculation and reasoning from other observations, are to be

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* For example, the first appearances and last disappearances of the sun's upper and *lower* border, above and below the sea-*horizon, if at* sea or on the coast,—or on land the exact length of the shadow of a vertical object of determinate length on an horizontal level, at a precise moment of time (not too near noon), &c.

TABLE of HOURLY METEOROLOGICAL OBSERVATIONS, made during the 21st and 22d of March, 1837, at BLACKHEATH ROAD, near Greenwich, about four-and-a-half miles to the south-east of London, by J. H. BELVILLE, in conformity with the Instructions circulated by the South African Literary and Philosophical Institution.

DAY.	HOUR.	BARO-METER.	THERMR.		WIND.		CLOUDS, &c.
			Attached	In open air.	Direction.	Strength	
		English Inches.	Fah. °	Fah. °			
Tuesday, March 21, 1837.	VI. A.M.	29·971	42	24·2	N. by W.	1	Snow on the ground. Intense frost. A few clouds to N. A Radiator on the ground had fallen to 19°.
	VII.	29·967	42	26·8	N.	1	Clouds coming up. Sunshine through thin clouds.
	VIII.	29·962	39	28·2	N.	1	Clouds collecting, and overspreading the sky. *Cirro-stratus.*
	IX.	29·943	36	29·6	N.	1	Clouds increasing. Snow coming on from the northward.
	X.	29·928	35	30·0	N.	1	Snowing. *Cirro-stratus.*
	XI.	29·903	35	29·0	N.N.W.	2	Small snow. [*muli.*
	XII.	29·892	36	31·2	N.	1	Clouds broken. *Cirro-stratus* into *cu-*
	I. P.M.	29·863	36	31·1	N.	1	Clouds broken. Snow still falling.
	II.	29·848	37	31·5	N.	1	A shower of granular snow has just fallen.
	III.	29·843	37	33·0	N.	1	Large dense *cumuli.* Clear sky be-
	IV.	29·836	39	33·4	N.E.	1	*Nimbus* advances from N. [tween.
	V.	29·839	38	31·5	E.	0	Shower of snow. Wind changes to E.
	VI.	29·841	39	29·8	E.S.E.	1	Snow, beautifully crystallized of a *hexagonal* form, falls, and remains on the ground.
	VII.	29·839	38	28·9	S.E.	1	Clearer. Moon visible.
	VIII.	29·838	42	28·7	S.E.	1	Clouds again collect from the N.
	IX.	29·830	39	28·6	S.S.E.	0	Getting quite overcast. Very cold.
	X.	29·830	42	28·4	S.	0	Generally cloudy, and calm.
	XI.	29·824	43	28·8	S.	0	The same.
	XII.	29·818	43	28·7	S.W.	0	The same.
Wednesday, March 22.	I. A. M.	29·808	43	28·7	S.	0	The *cirro-stratus* cloud prevails.
	II.	29·797	43	28·7	S.	1	Sky quite covered, as before.
	III.	29·798	43	28·6	S.	0	The same. Night extremely cold. Ground hard with frost. Ice nearly an inch thick.
	IV.	29·796	43	28·6	S.	0	The same.
	V.	29·796	42	28·0	S.W.	0	The same.
	VI.	29·798	42	27·9	S.	0	Clouds broken to the east. Sun visible at his rising. [quickly from N.
	VII.	29·814	42	28·5	S.E.	1	Clouds getting thinner, and moving
	VIII.	29·836	42	29·1	E.S.E.	1	Getting clear, particularly to the N.E.
	IX.	29·838	39	31·8	E.	1	Still clearer. Sun shining bright.
	X.	29·839	40	34·0	E.	1	Fine. *Cumuli* at a distance.
	XI.	29·842	41	36·1	E.	2	The same.
	XII.	29·858	42	38·4	S.E.	1	*Cumuli* collect apace.
	I. P. M.	29·863	43	37·7	S.E.	1	More overcast. [snow fall.
	II.	29·864	43	37·0	E. S. E.	1	*Cumuli* into *cum.-stratus.* Flakes of
	III.	29·866	43	36·8	S.E.	0	Thick. No blue sky visible.
	IV.	29·866	42	36·1	E.	0	The same. Upper atmosphere hazy.
	V.	29·870	42	33·0	E.	2	Clouds thinner in zenith. Air feels cuttingly cold.
	VI.	29·904	42	31·5	E.	1	Getting quite cloudy and thick.

NOTES.—The time was taken from a good clock, keeping nearly mean time, its rate being scarcely perceptible, and its error obtained by observation of the Ball-Drop of the Royal Observatory, Greenwich.

The Barometer has an elevation of about forty feet above the River Thames.—Makers, Watkins and Hill, London.——The Thermometer has a northern aspect.

The strength of the wind is indicated by the figures in the column, thus—0 marks that there was *no wind perceptible;* 1, *a very light breeze;* 2, *a strong breeze.*

reserved till the final discussion of the series, and for separate determination and statement*.

"5. If copies be taken of the registers, they should be carefully compared with the originals by two persons, one reading aloud from the original, and the other attending to the copy, and then exchanging parts, a process always advisable wherever great masses of figures are required to be correctly copied.

"6. The register of every instrument should be kept in parts of its own scale, as read off, no reduction of foreign measures or degrees to British being made—but it should of course be stated *what* scale is used in each instrument.

II.—*Of the Times of Observation and Registry.*

"Meteorological observations should be made and registered daily, at stated and regular hours. In fixing on these, some sacrifice of system must of necessity be made to the convenience and habits of the observer. The best hours, in a scientific point of view, would be those of Sun-rise, Noon, Sun-set, and Midnight. But these are not the hours adapted to general habits; and since the midnight observation is likely to be pretty generally neglected elsewhere than in an Astronomical Observatory, the following hours, for a division of the day into three parts, are proposed for what may be deemed the Morning, Afternoon, and Evening observations, viz.

Morning . . .	8 A. M.
Afternoon . . .	2 P. M.
Evening . . .	8 P. M.

"If, however, the habits or engagements of any one should not allow him to conform to these hours, rather than not observe he may select his own, specifying only what they are at the head of every page of his register, and adhering steadily to them in practice, only observing to make the extreme observations of each day equidistant from the middle one.

"At the same time it will be borne in mind, that, in what concerns the great meteorological questions on which the most interesting features of the subject depend, the night is quite as important as the day, and has hithero been far too much neglected. To any one, therefore, who may feel disposed to enter more zealously into the subject, and will not consider some personal inconvenience ill undergone for the sake of affording data of a peculiarly valuable description, this Committee would most earnestly recommend the adoption, in preference to all others, of the quaternary division of the 24 hours.

III.—*Of Meteorological Instruments,—and, first, of the Barometer and its attached Thermometer.*

"The Barometer is the most important of all meteorological instruments. Its office is to measure the actual pressure of the atmosphere on a given horizontal surface at the time and place of observation. Its fluctuations are observed to have considerable relation to changes in the weather, and especially of the wind. Hence its use as a weather-glass.

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* We regard this as of the highest importance.

"A barometer should be examined, before setting it up, for air-bubbles in the tube, and for the existence of air above the mercury in the upper part of the tube. This is done by gently inclining the instrument either way from the horizontal position a little up and down; when air-bubbles, if large, will be seen to run to and fro, and must be evacuated by inverting the instrument, and by gentle blows on it with the hand, driving them up into the cistern. If this cannot be done, the instrument is useless. If air exists to an objectionable amount *above* the quicksilver, it will not tap *sharp* against the upper end of the tube when the barometer is quickly inclined from a vertical position, so as to make the mercury rise above its level, nearly to the top, and then gently *jerked* lengthways and backwards. If the blow is puffy and dead, or is not heard at all, the amount of air must be considerable, and may be expelled by inversion.

"In fixing the barometer, choose a good light near a window, but not exposed to sunshine, in a retired apartment, little liable to sudden changes of temperature or to draughts of wind. Adjust the tube to a vertical position by a plumb-line, and fix it so as never to shift from that position. Before reading off, give a few taps on the instrument, enough to make the upper end of the column of silver *shake* visibly, as the mercury is apt to adhere to the glass, and give erroneous readings. In reading, bring the index always opposite to one part. The correct part to choose is the summit of the convexity of the mercury, to which the index should be made a tangent; but if this be difficult to hit, either from the construction of the index or the want of a proper fall of light, the line of junction of the mercury and glass may be taken. In that case, the tapping should never be omitted. Whichever mode of reading is once adopted, should be stated, and always adhered to. A piece of white paper placed behind the upper part of the tube will generally enable any one to read off by the convexity of the quicksilver. In placing the index, notice whether it appears to shift a little up and down as the eye is raised or depressed: this is called parallax, and is a source of uncertainty, to be avoided by placing the eye in reading always on the *exact level* of the top of the mercurial column.

"Barometric observations require corrections of three kinds; and to render them available and comparable with others, it is necessary that their amount should be ascertained, and distinctly stated. The first is called the Zero Correction. It includes several subordinate corrections arising from different sources, such as that originating in a faulty placing of the scale of inches, that due to the capillary depression of the mercury in the glass-tube, and the constant part (which at a fixed station is nearly the whole) of the depression arising from the presence of air or vapour in the upper part of the tube.

"To determine the zero correction, the barometer must be compared with a standard instrument, such as that at the Royal Observatory for instance, or some other which has been compared with it, or with some standard of equal authority. Such comparison ought never to be omitted before forwarding the barometer to its place of destination, nor should any opportunity be neglected of comparing it, when fixed in its place, with a good portable barometer. In making such comparisons, all that is necessary is to record the readings of both the instruments, after at least an hour's quiet exposure, side by side, that they may have the same temperature. If compared by two observers, each should read off his own barometer in his usual manner, and each should take a mean of several readings, then each should verify the other's results. By this means the zero of one standard may be transported over all the world, and that of all others compared with it ascertained.

"The amount of the zero correction is often very large, as two or three tenths of an inch, but its influence on the mean results of recorded observations, falls wholly on the determination of the heights of the station of observation above the mean level of the sea, and affects little, if at all, any conclusions of a meteorological nature which may be deduced from them. Hence, if proper care be taken to preserve a barometer, once set up, immoveable, a long and regular series of observation with it has a value independent of any knowledge of this element, and it is fortunate that this is the case, as the zero correction is one extremely difficult to determine exactly *à priori.*

"In transporting a *compared* barometer to its place of destination, great care is necessary. It should always be carried *upright*, or considerably inclined, and *inverted*, and over all rough roads should be carried in the hand, to break the shocks to which it would otherwise be exposed. Strapped obliquely across the shoulder of a horseman, however, it travels securely and well, and with common care in this mode of transport, its zero runs no risk of change.

"The next correction, and the most important of all, is that due to the temperature of the mercury in the barometer-tube at the time of observation. To obtain this, every barometer requires to have attached to, or fixed very near it, a thermometer, called the attached thermometer, which must be read and registered at each observation of the barometer. It is preferable in practice to read off this thermometer *first*, to avoid the error arising from breathing on, or standing long near it, while reading the barometer itself. The zero of this thermometer should be ascertained by comparison with a standard at the temperature of about 60° Fahr.

"The third correction applicable to barometric observations arises from change of level of the mercurial surface in the cistern, owing to the transfer of a portion of its contents to or from the tube. In barometers with small cisterns, and where the lower level cannot be adjusted at each observation, its amount may be large, and its effect being always to make the apparent fluctuation less than the real, *in a fixed proportion*, it ought, if possible, to be ascertained. The data necessary to be known are—first, the internal and external diameters of the tube—secondly, that of the cistern containing the mercury, at the surface, where the tube plunges into it. These particulars, as they must be known to the maker, ought be inquired of him, and indeed ought to be engraved conspicuously on some part of the instrument.

"*Although* all these corrections are necessary for the strict *reduction of registered* observations, they ought not to be applied to individual observations previous to registry. It is sufficient to know them. Their effect is in all cases easily and safely applicable to mean results, and to the conclusions therefrom deduced, and a world of troublesome and often mistaken calculations may be saved by so applying them.

"*Of the External Thermometer.*—The external thermometer should have a scale on which whole degrees are read off, and divisions large enough to admit of estimating tenths, or at least quarters of degrees, by the eye. It should be compared with a standard, and the difference stated, at one or more temperatures (the wider asunder the better) within the range of the climate in which it is to be used. In fixing it, choose a perfectly shaded but otherwise free exposure, and one where no *reflected* sunbeams from water, buildings, rocks, or dry soil, can reach it, and easily accessible for reading. There fix it firmly and upright. In reading it, avoid touching, breathing on, or in any way warming it, by near approach of the person. The quicker the reading is done the better.

"Although read off at stated times, notice should be taken of all sudden and remarkable changes of temperature, as indicated by the external thermometer, whenever they occur.

"*Of the Maximum and Minimum, or Self-registering Thermometer.*—This should be placed horizontally in some place out of doors, shaded from direct radiation and rain, and otherwise freely exposed to air, and so fastened as to allow of one end being detached from the fastening and lifted up, so as to let the indexes within the boxes slide down to the ends of the fluid columns, a more convenient mode, when the steel index is free enough to allow it, than the use of a magnet.

"Both the thermometers should be read off as early as possible every morning, and the indexes re-adjusted. But as double maxima frequently, and occasionally double minima occur, in consequence of sudden changes of temperature, it is recommended occasionally to inspect both of them, with a view to ascertain whether the motion of either the mercury or spirit has been reversed in an unusual manner; and such double maxima or minima, when remarkable, should be recorded as 'supernumerary,' with their dates and leading features*.

"The Self-registering Thermometer is extremely apt to get out of order, by the indexes becoming entangled in the column of fluid. In travelling, they should not for a moment be carried with the mercury bulb downwards; if this should happen, they are *sure* to arrive in a state unfit for use. To correct them is tedious, and always hazards fracture. With great care, however, it may be done, as follows:—

"1*st.* The Spirit Thermometer. By many jerks, force the index down to the junction of the bulb and tube; then, by cautiously heating and cooling alternately the bulb, the tube, or the air-vessel at the top, as the case may require, the disunited parts of the spirit may be *distilled* from place to place, till the whole is collected in one column in union with the spirit in the bulb.

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* "The spirit thermometer is apt to undergo a gradual change of zero by the transfer (by distillation) of part of its spirit to the upper end of the tube. It should, therefore, often be compared with the mercurial one, and the difference of readings *applied* as a zero. In this *only* case is the application of a zero *before* registering permissible, and indeed essential."

Springvale, Maine.—*Mr. Dunster.* "At three o'clock I began to count the meteors, and numbered as follows.

Time.	Number.
3 h. 30 m.	37
3 h. 45 m.	25
4 h.	31
4 h. 15 m.	25
4 h. 30 m.	22
4 h. 45 m.	28
5 h.	22
5 h. 15 m.	16
5 h. 30 m.	20
5 h. 45 m.	11
6 h.	11
6 h. 15 m.	5
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"The meteors, with the exception of five or six, all had a direction from a point in the eastern part of the heavens about 15 degrees N.N.E. of the planet Jupiter; and, although they appeared in all parts of the sky, still, if the lines of motion had been continued backwards, they would all have terminated in that point. Having witnessed the meteoric shower of 1833 in Pennsylvania, I was particular to observe the foregoing fact. The phenomenon appeared to me to be identical with that, but far less magnificent. The day preceding had been remarkably rainy, but the night was clear and still.

"Between four and five o'clock, an *auroral arch* was to be seen in the north, and *streamers* at half past five."

Cambridge, Mass.—"At eighteen minutes before four o'clock a large meteor darted from the north. It was quite luminous, and in size apparently equal to half the full moon. This was succeeded by many smaller meteors, and twenty-three were counted by me during an hour and a half; several were seen by other persons *in the room*[*], which escaped my notice. During this time one was observed of great brilliancy, having a luminous train apparently a yard in length. The lightning[†] continued the whole time, and there was considerable appearance of Aurora Borealis. W."

Yale College.—The preceding day had been rainy, and early the same night the sky was overcast; but before midnight, the firmament became cloudless, and the stars shone with uncommon brilliancy. My expectation of a repetition of the meteoric shower at this place was so slight, that I had made little preparation for observing the heavens, although I looked out frequently after midnight. About half past three o'clock, finding that the meteors began to appear in unusual numbers, I directed my attention towards the eastern part of the heavens, whence they appeared mostly to proceed, and closely watched the stars from the Great Bear on the north to Canis Major on the south, embracing in my field of view about one-third of the firmament.

It was soon discovered that nearly all the meteors shot in directions which, on being traced back, met in one and the same point near the eye of Leo. For a quarter of an hour from half past three o'clock, I counted twenty-two meteors, of which all but three emanated from the above radiant point. Ten left luminous trains; twelve were without trains; and the three that did not conform to the general direction, moved perceptibly slower than the others. The greatest part shot off to the right and left of the radiant, the majority tending south towards the heart of Hydra. The next fifteen minutes afforded but seven meteors, and the number gradually declined until daylight.

The exact position of the radiant was near a small star forming the apex of a triangle with the two bright stars on the face of Leo, having a right ascension of 145, and declination of 25 degrees[‡]. Its place, therefore, was very nearly the same as in 1834, differing only half a degree in right ascension; and all the phenomena very much resembled those observed that year, except that they were on a scale somewhat inferior.

New York.—"The annual recurrence of this phenomenon being a subject of much interest, the undersigned kept a careful watch on the night of Saturday, and morning of Sunday last, and is gratified in being able to announce the re-appearance of this phenomenon with considerable brilliancy.

"During the evening but few meteors were observed, but from eight o'clock until near the dawn, successive flashes were observed in the east, supposed by some to be lightning. At eight o'clock, a very beautiful aurora-light was seen, of a pinkish colour. This continued for a short time only, although a general luminous appearance in the north remained during the night.

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[*] From this expression it is inferred, *that the writer had but a small portion of the firmament in view.*

[†] *From light clouds in the S. E.*

[‡] This position of the "radiant," as observed here in 1833, was in R. A. 150°, Dec. 20°; in 1834, R. A. 144° 30′, Dec. 30° 15′.

"About two o'clock in the morning, several meteors were seen to dart across the Great Bear, and from this time constant watch was kept up until daylight. From two to three o'clock, ninety-eight meteors were counted, some being very small, but the greater number of great size and brilliancy, resembling a rocket both in the explosion and trail left behind,—the trails lasting in some instances for nearly two minutes.

"With two or three exceptions, the course of the meteors was divergent from a point in Leo, Declination 20°. Right Ascension 150°, nearly. The place of this point was fully confirmed during the night.

"From three to four o'clock, one hundred and fifty meteors were counted, and three hundred in all were enumerated. After this time we kept no account of the number, though many more appeared. From the situation of the observer it is probable that more than half escaped notice. Several were seen in the clear light of the dawn; and Jupiter, Venus, and Mars, all shining with great brilliancy, were alternately outshone by these transient rivals. No doubt now exists in the mind of the writer, as to the distinct and peculiar character of the phenomenon; for, though an attentive observer of such matters, he has never seen anything bearing the slightest resemblance to this display, except on the night of Nov. 12—13th, 1832, when he had the good fortune to observe the same appearance while at sea, off the harbour of Pernambuco, one year before the far-famed shower of 1833."—G. O. S.

NEWARK, NEW JERSEY.—"This account much resembles the foregoing, as might be expected from the proximity of the two places of observation. The writer remarks, that previous to two o'clock a few shooting stars were seen, but no more than on ordinary occasions. After that, however, there was a decided increase. In an hour and a half he counted above seventy-five, although his field of view took in only 60 degrees. After four o'clock, their succession was less frequent, and they continued to diminish in number until the dawn of day. He thinks the whole number that fell was not less than four hundred."

RANDOLPH MACON COLLEGE, VIRGINIA. By *Prof. R. Tolefree.*—"On the night of the 12—13th November, three of the students and myself prepared to watch all night. The sky was serene, and all was calm. About ten o'clock meteors began to appear. The first distinguished for its brilliancy, started from the lower part of the Little Bear and proceeded to the south-west. After midnight until two o'clock, all the meteors shot westward; and from two o'clock until day-break their course was entirely north-west. We only watched occasionally during the night, and only on the northern side of the heavens, except an occasional visit to the other parts of the building*. I counted two hundred and forty-eight shooting stars, and my companions saw a larger number than this. You may safely conclude that five hundred were seen by us, and this from observations kept up only at intervals during the night."

SOUTH CAROLINA. GREENVILLE, *Nov. 19th.*—"We learn that the people in the neighbourhood of Maybinton, Newbury District, witnessed the fall of an immense number of meteors, which first made their appearance about twelve o'clock on Saturday night last, and continued their descent until daylight the next morning. It is said their number was not near so great as that of the 'Falling Stars' three years since; but the spectacle is represented as having been very brilliant and unusual."

From the foregoing accounts compared, we are led to conclude that the meteoric shower increased in intensity from north to south, that in South Carolina having been the most considerable of all, so far as accounts have reached us.

Does not the recurrence of this phenomenon, for six successive years, at the *same period of the year*, plainly show its connexion with the progress of the earth in its orbit? and does not the fact, that the greatest display occurs everywhere in places differing widely in longitude, at the *same hour of the day*, as plainly indicate its connexion with the motion of the earth on its axis? The supposition of a body in space, consisting of an immense collection of meteors, stretching across the earth's orbit obliquely, so that the earth passes under it in its annual progress, while places on its surface, lying westward of each other, are successively brought by the diurnal revolution to the point of nearest approach, will satisfy both these conditions. I can think of no other that will.

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* "Had Prof. Tolefree taken his station where the view of the firmament would have been unobstructed, he would probably have seen a still greater number shooting to the south-west."

The "point of nearest approach" may be merely the extremity, or the *skirt* of the nebulous body, while the greatest part of it, and consequently its centre of gravity, lies too distant from the earth to be much influenced by its gravity. It would not be at all inconsistent with the known extent of astronomical bodies, to give to the body in question a breadth of thousands, and a length of millions of miles.

It was an accidental observation, made after the conclusion was formed, which ascribes the origin of the meteoric showers to a revolving nebulous body, that first led me to suspect the *Zodiacal Light* to be the body in question. This, according to La Place, *is* such a nebulous body, revolving around the sun in the plane of the solar equator[*]. We actually observe it to reach over the orbit of the earth, making an angle with its plane of only 7¼ degrees. It is not difficult to place it in such a situation that the earth shall come very near to the *skirts* of it at least. We should, indeed, expect this meeting of the two bodies to take place at the nodes of the solar equator, and, therefore, in December and June instead of November and April. It is easily conceivable, however, that the aphelion of the Zodiacal Light, at which place it approaches nearest the earth, does not lie exactly at the node, but so far from it, that the earth passes it a month before it comes to its node, at which time, moreover, the earth is more than a million of miles nearer to the sun than its mean distance. In endeavouring to fix the *periodic time* of the meteoric body, since it must be either a year or half a year, (for no other periodic times could bring the two bodies together at intervals of a year[†],) several considerations induced the belief, that *half a year* was the true period—an inference drawn especially from the apparent great excess of velocity of the earth at the point of concourse; but the period of *a year*, (or more probably, a little less than a year,) by implying that the two bodies are always comparatively near to each other, would better explain the occurrence of shooting stars at all seasons of the year, and would be particularly favourable to the explanation of those meteoric showers which have, on two occasions at least[‡], occurred near the last of April,—a time distant about half a year from November, and therefore sustaining a like relation to the opposite point of its orbit. In such a case, meteoric showers would occur in April and November, for the same reason that the transits of Mercury take place in May and November exclusively. The greater frequency of meteors in November than in April, naturally results from the greater proximity of the earth to the sun at the former than at the latter period; to which, perhaps, may be added, the effect of the eccentricity of the orbit of the meteoric body, the aphelion being on the side of November. In the present state of our knowledge on this subject, I regard it as a point open for inquiry, whether it will best accord with all the phenomena of shooting stars, to give to the meteoric body a period of nearly one year, or of half a year."—OLMSTED. *Silliman's Journal.*

Correct Latitude of the Brussels Observatory.

"ONE of my first objects, as soon as I was able to make use of the instruments in the Observatory of Brussels, was the endeavour to determine the geographical position of that establishment. In order to obtain the latitude, I used the mural circle, made by Messrs.

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[*] *Méc. Céleste,* (Bowditch,) Vol. II., *p. 425.*

[†] See Vol. XXVI., p. 166, *Silliman's Journal.*

[‡] "In Virginia, and various other parts of the United States, in 1803, and in France, in 1095. Making suitable allowances for the more rapid progress of the earth through the winter signs, and for the change of style, and the meteoric shower of the 25th of April, 1095, occurred at very nearly the opposite point of the earth's orbit."

Troughton and Simms. It was not until the end of July, 1835, that this instrument had been fixed, and which event we owe to the obliging services of Mr. Gambey, who was then at Brussels, for the purpose of fixing his transit instrument. The circle is the exact copy of that of Greenwich; it is six English feet in diameter, and has two graduations, the one on gold, and the other on palladium. It has six microscopes, by which seconds may be read off, and their fractions estimated."

(M. Quetelet then describes his manner of observing, his calculations, and the results of his numerous observations.)

"I am of opinion, after all that I have detailed, that the latitude of the Observatory at Brussels may be taken at 50° 51′ 10″ 7[*].

"However satisfactory the coincidence of the result of the preceding operations may be, it ought not to be concealed, that since the determined latitude rests upon observations of the same star, it is possible that it may be tainted with an error, constant to this star, and depending upon a flexion of the instrument, or some other cause acting always in the same direction; though I believe such a cause of error to be little probable."—Quetelet, *Sur la Latitude de l'Observatoire de Bruxelles*, 1836.

Holidays well Spent.

The following gratifying result attended the successful efforts of the parties who advocated the opening of the British Museum to the public, during the Easter week just passed:—

The visiters admitted on Easter Monday, were	23,985
— Tuesday — . .	2,972
— Wednesday — . .	5,029
— Thursday — . .	1,543
— Friday — . .	2,696
Total	36,225

G. Samouelle, *one of the officers of the British Museum.*

At the Adelaide-street Gallery for the Illustration and Encouragement of Practical Science, more than 1400 persons paid for admission on Easter Monday.

Antiseptic Properties of the Essential Oils.

"I found that the essential oils of cinnamon and cloves possessed an antiseptic power, quite equal to that of kreosote, and that their aqueous solutions, when sulphated, were even superior to similar solutions of that agent.

One part of milk mingled with four parts of a saturated aqueous solution of the sulphated oil of cloves, remained, after five days, sweet and liquid, while another portion of the same milk became curdled and sour within twenty-four hours. Having on the 2nd day of July added two drops of oil of cinnamon to an ounce measure of fresh milk, it remained liquid on the 11th; and, though it finally coagulated, it continued free from bad taste or smell till September, although other portions of the same milk had become putrid. A half ounce of milk, to which a drop of sulphurous oil of turpentine had been added, remained free from coagulation at the end of two days, while another portion, containing five drops of pure oil of turpentine, became curdled and sour on the next day.

A number of pieces of meat were exposed in small wine-glasses, with water impregnated with solutions of the various essential oils. Their antiseptic power seemed to be in the ratio of their acridity. The milder oils seemed to have comparatively little antiseptic power, unless associated with the sulphurous acid, which has long been known as an antiseptic.

In cutaneous diseases, and, perhaps, in the case of some ulcers, the employment of the sulphurous sulphated oils may be advantageous.

A respectable physician was of opinion that the sulphurous sulphate of turpentine had a beneficial influence in the case of an obstinate tetter.

Possibly the presence of sulphurous acid may increase the power of oil of turpentine as an anthelminthic.

Pieces of corned meat hung up, after being bathed with an alcoholic solution of the sulphurous sulphated oil of turpentine, or with solutions of the sulphated oils of cloves or cinnamon, remained free from putridity at the end of several months. That imbued with cinnamon had a slight odour and taste of the oil.

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[*] The latitude of Brussels was given in the *Connaisance des Temps de Paris*, for 1727, as 50° 51′ 00″.

In the same work for 1789, it was stated to be 50° 50′ 59″, and this has been annually repeated up to the period of M. Quetelet's *observations.*

I am led, therefore, to the impression that the antiseptic power is not peculiar to kreosote, but belongs to other acrid oils and principles, and especially to the oils of cinnamon and cloves."—HARE. *Silliman's Journal.*

Aqua versus Quaquaversus.

"Who shall decide when Gall'ries disagree?"

OUR early subscribers, and, at the present moment, an immense number of the public, of all ages, and sexes, and conditions, are familiar with, the appellation at least, of "THE GALLERY OF PRACTICAL SCIENCE." Its more formal and stately title is that of "THE GALLERY FOR THE ILLUSTRATION AND ENCOURAGEMENT OF PRACTICAL SCIENCE," but the former is the portable form by which it usually circulates.

The proprietors of a publication, dated "[March 1st, M.DCCC.XXXVII.[*]]," trying the side and short-cut road to Notoriety, if not to Reputation, have adopted, without remark or apology, this distinctive name of the above establishment, and in German Text, or Old English, have labelled an octavo pamphlet, thus—

The Gallery of Practical Science.

Not to rank this assumption of an already existing designation among the deceptive tricks of trade, it may be censured on account of the temporary inconvenience and confusion it produces. The substantial Gallery of *Brick and Mortar* has acquired a *solid* and honourable reputation, for the facility it has given to experimenters, by the free use of its costly and powerful apparatus, and its ample space. Some of the experiments made within its *walls*, have enabled scientific students and inquirers to examine many doubtful but important questions and theories, and to give to each, for the first time, their proportional merit. Several of them, made on the canal of this establishment, we know to be at this moment under the consideration of the Institute of France; many others have been published, and are frequently appealed to.

Now the *Paper* Gallery, (which may, without sarcasm, be called in contradistinction to its predecessor, the *superficial* one,) contains a large number of "EXPERIMENTS" within its *covers*, and certainly gives many well-drawn and well-engraved diagrams to illustrate them. There also may be, in one or two of the experiments, a little novelty, but in general they are, and necessarily must be, experiments made many years before either of the Galleries was projected, and most of them are described in every work which treats of the physical properties of Fluids. But here lie the inconvenience and confusion complained of; differing, as the nature of the "Experiments" in the two Galleries really does, yet both are now called, in common parlance, Experiments of the Gallery of Practical Science, and hence, owing to the liberal circulation of the Pamphlet Gallery, assertions the most positive are made that experiments have been made in the other, corroborative of a class of assumed facts, and vulgar paradoxes, which it has always been the distinction of the latter Gallery and its frequenters, to examine and extirpate. For a striking illustration of this, we shall give a recent instance. During the last five or six weeks, it has been matter of scientific gossip and wonder, that the Gallery of Practical Science (observe, it was not added whether in octavo or in stone,) had proved *that living fishes in water could sustain "pressure from without," to any amount*, and it was also stated, that the Gallery of Practical Science (without addition, as before,) had proved, *that living fishes in water were instantaneously killed under the same circumstances.* Very few persons have either the taste or the opportunity to ascertain which of such contradictory statements is true, and to trace out the origin of the conflicting assertions. In the present instance, it fell to our lot to be able, as we were willing, to enter into both Galleries, and, bringing out the respective experiments relating to the question, to exhibit the cause of the concurrent existence of the contradiction, and to dissipate the confusion produced by the latter Gallery of Practical Science. As one, at least, of the actual experiments was a decisive fact, and in direct opposition to one of those printed in the book, we shall lay the case before our readers in the way we did that of the Manchester and Newcastle Institutions, that is, side by side, for the advantage of the contrast.

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[*] Mechanics of Fluids and Hydraulic Architecture, descriptive and constructive, the *whole illustrated* by scientific Examples and appropriate Diagrams. London, Edwards, *12, Ave-Maria Lane.*

The Gallery of Practical Science,

12, Ave-Maria Lane, London.

In this "Original Work, conducted by professional, scientific, and practical men, Part I.", dated [" March 1st, M.DCCC.XXXVII."], but to which no name, except those of the wood-engraver and the publishers are attached, though that of the celebrated Professor Jameson has freely been made use by some of the vendors of the book, there occurs at p. 155 the following subdivision:—

"Experiments illustrating the Quaquaversus Pressure of Incompressible Fluids."

And this "quaquaversus" is thus introduced.

"Before we conclude our inquiries on fluid pressure, it may be both interesting and instructive to the readers of this work, to describe a few select experiments, by which the equal distribution of pressure, among the particles of an incompressible fluid is beautifully and rigorously demonstrated, and its equal propagation in all directions placed beyond the possibility of the smallest doubt."

Under Experiment are the following assertions and conclusions:—

"If a living fish should be put into the cylinder of a hydrostatic press, when under a very high degree of pressure, it will not suffer the least inconvenience; from which it is obvious, that every particle of the fluid is equally pressed, and presses equally in all directions.

"Numerous other examples might be adduced for proving the same thing, but since the principle is manifest, it is needless to dwell longer on the subject." p. 160.

The Gallery of Practical Science,

Adelaide Street, Strand, Westminster.

In the long room of this Gallery is one of the most exquisitely-constructed hydrostatic presses that the English skill of the present day could execute. It was made under the direction of an eminent mechanician, in a manufactory remarkable for the perfection and accuracy of its workmanship, and no cost was spared by the proprietors of the Gallery. A pressure of 30,000 lbs. upon the square inch can be produced with ease.

In this machine, it has been repeatedly demonstrated that water *is not an incompressible fluid.*

Into this machine a live eel was introduced, and subjected for about a quarter of a minute to "*a very high degree of pressure.*" The tenacity of life in eels is proverbially remarkable; so much so that they seem to be entirely exempted from "sudden death." But to the case of the eel in the press this exemption did not reach; for when lifted out he was "dead, dead, dead!" We believe no eel, ready-skinned and decapitated for the purpose, was ever finished before in fifty times the time.

In the Gallery are some unrivalled means of exciting and applying that extraordinary agent which has made even the executed criminal writhe and start up hours after he had been cut down from the gibbet; but, although within a minute of its death and exhumation, the eel was subjected to several modes by which galvanic influence of great energy is rapidly produced, every application of it was vain; the eel was perfectly insensible and rigid; it never moved a muscle, nor made the slightest attempt to slip through the fingers!

By "this picture and this," we hope we have winged and brought down one of that numerous class of misrepresentations, which are copied by compiler after compiler, and published in book after book. Public establishments of the nature of the Adelaide-Street Gallery, by making experiment easy, would soon sweep out the whole of the "vulgar errors" which remain.

Watch Statistics.

Mr. Dent, (Arnold and Dent,) in his illustrations of a lecture on the construction of watches and chronometers, given by him at the Royal Institution on the 7th ult., laid before the meeting the dissection of a detached lever watch (compensation-balance,) every part was separated and displayed, but grouped in one of six larger divisions to which it belonged.

Each part had been previously examined, and its distinct constituent pieces counted by the lecturer; the surprising result of this enumeration was exhibited in a table, of which we lay a copy before our readers. In addition, will be found the number of kinds of artificers concerned in the operations necessary for the construction of a good watch. When to these are added the amount of previous operations which the materials constituting each piece must undergo, *before* it comes into the hand of the watch-artificer, a glimpse may be obtained of the extensive and numerous changes of form and value which "raw material" receives in its progress, from the mine to so refined a manufacture as a finished watch.

No. of Parts.	No. of Pieces.	Trades employed.
1. Pillars	4	1
2. Frame	4	1
3. Cock and Potence	2	1
4. Barrel and Arbor	3	1
5. Going-Fuzee	14	2
6. Wheels	4	1
7. Pinions	4	2
8. Stop-Stud	1	1
9. Stop and Spring	3	1
10. Click and Ratchet	3	1
11. Motion	16	2
12. Jewels (5 Holes)	28	2
13. Cap	3	2
14. Dial	5	3
15. Index	1	1
16. Escapement	13	3
17. Compensation-Balance	9	1
18. Case	3	1
19. Pendant	2	1
20. Case-Joint	6	1
21. Case-Spring, &c.	4	2
22. Main-Spring	1	2
23. Chain	826	3
24. Hands	3	1
25. Glass	1	1
Total of Pieces	992	
Engine-Turner		1
Engraver		1
Gilder		1
Examiner		1
Total of kinds of Artificers employed		43

The whole (in a Lunar Eclipse) greater than the Total.

Some young students of Astronomy, though of adult age, were, during the late eclipse of the moon, very much puzzled with the dimensions of the eclipse which were given in several of the almanacs. To them it was incomprehensible that any eclipse of the moon could be greater than the moon iself, but even in the *Nautical Almanac*, that volume of almost pure truth, they so found it stated. Thus it stands at p. 465:—

"Magnitude of the Eclipse (Moon's diameter = 1) 1.659, on the southern limb."

The ignorance which used to be ashamed of inquiry, and which was, therefore, irremoveable, is, we are glad to say, disappearing rapidly, and the class of persons who are opening their mouths for instruction, is as rapidly increasing. To some of this class, we are indebted, and really so, for the above statement of their difficulty, and to them we are happy to tender the following definition.

The magnitude of an eclipse of the moon is the diameter of the earth's shadow which passes over her, expressed in terms of the moon's diameter. Thus, in the late *eclipse*, the diameter of the shadow was larger than that of the moon in the proportion of about 16 to 10. Great as is the authority which calls this the magnitude of an eclipse of the moon, we agree with our correspondents, that it has far more of technicality in it than of common sense *.

The late Astronomer-Royal; and the Lalande-Medal.

In several of the biographical notices of the late Mr. Pond, and, it is particularly to be regretted, in that given by the President of the Royal Society, in his address to the fellows, in November last, no mention has been made of the Lalande-medal which the astronomer received from the French Institute. That this omission was not just to either party, must be evident, when it is known that the particular service he rendered to astronomical knowledge, merited a notice from the author[*] of the address in such terms as the following:—

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* We refer our more juvenile correspondents, who inquire what effect the eclipse had upon the eyes, nose, and mouth of the queen of night? whether she blinked, sneezed, &c.? to Mr. Rogerson, of the Royal Observatory, Greenwich, who knows all about these particularities, and has ten times over most faithfully delineated them from the life, in his Almanac for the present year, p. 27.

"The method which was adopted by Mr. Pond to determine the limits of the annual parallax of certain fixed stars by means of fixed telescopes of great focal length, was singularly ingenious and complete. The existence and amount of such a parallax had been asserted and assigned by Dr. Brinkley, in *α* Lyræ, *α* Aquilæ, and *α* Cygni; but this opinion, though most ingeniously and even obstinately vindicated and maintained by him, was, in the judgment of most other astronomers, most decisively negatived by Mr. Pond, who showed that the parallax of those fixed stars, supposing its amount to be sensible, was confined within the limits of the errors of the most delicate and perfect observations which have been hitherto made. There is no great question in astronomy, the present position and limits of which are more satisfactorily settled[†]."

Mention of the grant itself, independent of the personal honour it conferred upon Mr. Pond, was due to the illustrious Society who had decided in favour of an astronomer,—a foreigner to France. A communication from M. Arago to the late Astronomer, found among the papers of the latter, is so honourable to the parties then concerned, that we give it verbatim.

Du Lundi, 9 Mars, 1817.

"Voulez-vous, Monsieur et cher confrère[‡], me permettre de vous announcer que l'Institut, dans sa séance d'aujourd'hui, et sur la proposition d'une commission dont le hasard m'avait fait président, vient de vous décerner la médaille fondée par M. de Lalande.

"C'est tous vos intéressans travaux sur la parallaxe annuelle des étoiles qui ont principalement déterminé son choix. Cette nouvelle me fait trop de plaisir pour que je n'attache pas quelque importance à être le premier à vous annoncer: aussi j'envoie sans plus retard mon billet à la poste.

Mille et mille amitiés,
F. Arago.

"P.S. Présentez, je vous prie, mes hommages respectueux à Madame Pond et à Mademoiselle sa sœur."

Prize Subjects, 1837. *Institute of British Architects.*

The prize subjects of the Institute of British Architects for the present year are the following:—

1. On the excellences which distinguish the ancient Athenian architecture, and on the principles of art and science by which they were attained, with regard to design, proportion, light, shade, colour, construction, and adaptation to purpose, to situation, and to the materials employed.

2. On the system and principles pursued by the Gothic architects, from the 11th to the 15th centuries inclusive, in the embellishment by colour, of the architectural members and other parts of their religious and civil Edifices.

3. On the progressive improvement made during the last hundred years in the theory and practice of Construction, illustrated by diagrams and references to buildings.

4. For the best restoration of some Priory, Abbey, or similar class of Conventual Building: such as Fountains, Eastby, Rivaulx, or Kirkstall Abbey, Yorkshire,—Castle Acre Priory, Norfolk,—Llanthony Abbey, Monmouthshire, &c., to be drawn from actual measurement, distinguishing in a marked manner the parts existing, and those restored to complete the combination of the halls, apartments, refectory and offices; accompanied by a description.

The competition is not confined to members of the Institute.

Candidates are to deliver in their performances on or before the noon of the 31st of December of the present year. The conditions, &c., may be obtained from the secretaries, at the rooms of the Institute, 43, King-street, Covent Garden.

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[*] Said to be Professor Peacock, of Cambridge.

[†] Address delivered at the Anniversary Meeting of the Royal Society, on Wednesday, November 30th, 1836, by his Royal Highness the Duke of Sussex, K. G., &c., &c., &c., the President. London, Taylor, 1836.

[‡] Mr. Pond had been elected on the 14th of October in the preceding year, a corresponding Member of the Institute. The vacancy caused by his death has been recently filled by the appointment of Mr. Baily.

Hope deferred. Second Series.

A Journal of the Patents for Invention-Bill. Session 1837 !

14th Feb.

Patents for Inventions.—Bill to alter and amend the Patent Laws, and for better securing to individuals the benefit of their inventions, *ordered* to be brought in by Mr. Mackinnon and Mr. Baines.

15th Feb.

Patents for Inventions' Bill, "to amend the practice relating to Letters-Patent for inventions, and for the better encouragement of arts and manufactures," *presented* and read 1°; to be read 2° on *Wednesday*, 1st of March, and to be *printed*.

1st March.

Patents for Inventions,—Second Reading *deferred* till *Wednesday* 12th of April.

12th April.

Patents for Inventions' Bill—Second Reading *deferred* till *Wednesday* 17th of May.

Such is the rate of progress of this important Bill, which

> "like a wounded snake, drags its slow length along!"

Culpable Homicide, and subsequent Robbery, by the Florentine Government.

Sig. Segato, whose extraordinary preparations of the human body, and other animal substances, in a manner which suspends their decay, have excited so much curiosity, and of which we gave account*, died lately at Florence. As he never disclosed the process, his secret perished with him. His death is said to have been hastened by vexation, at the refusal of the Florentine Government to assist him in his undertaking. Now, however, so high a value is set by this Government on the preparations left by Sig. Segato, that they have prohibited the removal of them from the country!

Patent-Law Grievance. No. XIV.

The inventors of this country, and the introducers of inventions of other countries into this, were obliged to pay down to the attorney-general and other agents, &c., of the Government during the ten years ending December, 1834, more than £313,000†, and during the past year above £42,000 (being at the rate of £420,000 in ten years.) It is still to be observed that these enormous extortions are *exclusive* of the drawings, engrossings, and charges on each patent of the patent-solicitor.

The penalties inflicted on the inventive genius of Britain during the *present* year, up to the 25th ult., in the shape of government stamps and fees on patents, amount to more than £12,000!

N.B. This sum has been paid in *ready money* on taking the first steps, and as many of the inventors are poor men, (*Operatives*,) and a great many others of them persons to whom it would be very inconvenient to pay at least £100 down, they have been obliged to go into debt, or mortgage or dispose of their inventions, either wholly or in part, &c.

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* Vol. I., p. 335.

† See the detail of this sum, p. 238, taken from the Appendix to Mr. Mackinnon's Speech in the House of Commons, 14th Feb., 1837.

NEW PATENTS. 1837.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second that on or before which the Specification must be delivered and enrolled.——The abbreviation *For. Comm.*, signifies that the invention, &c., is "a communication from a foreigner residing abroad."

March *cont.*

62. **Joseph Haley**, Manchester, *Lanc.*, Machine Maker; for improvements in the machinery, tools, or apparatus, for cutting, planing, and turning metals and other substances. March 28.—Sept. 28.

63. **Joseph Whitworth**, Manchester, *Lanc.*, Engineer; for improvements in machinery, tools, or apparatus, for turning, boring, planing, and cutting metals and other materials. March 28.—Sept. 28.

64. **Henry Stephens**, Stamford-st., Blackfriars, *Surry*, Writing-fluid Manufacturer; for improvements in inkstands or inkholders, and pens for writing. March 28.—Sept. 28.

Total, March...21.

APRIL.

65. Michel Berand Lauras, Lyons, but now of Leicester-sqr., *Middx.*, Merchant; for improvements in steam-navigation. April 4.—Oct. 4.

66. Henry Booth, Liverpool, *Lanc.*, Esq.; for improvements in the construction of locomotive engine-boiler furnaces, applicable also to other furnaces. April 4.—Oct. 4.

67. William Wynn, Dean-st., Soho, *Middx.*, Clock Maker; for improvements in apparatus for diminishing the evaporation of vinous, alcoholic, acetic, and other volatile vapours, and for preventing the absorption of noxious effluvia in vinous, spirituous, acetous, and other fluids, such as wines, spirits, malt liquors, cyder, perry, and vinegar. April 4.—Oct. 4.

68. Joseph Amesbury, Burton-crescent, *Middx.*, Surgeon; for apparatus for the relief or correction of stiffness, weakness, or distortion in the human spine, chest, or limbs. April 4.—Oct. 4.

69. William Weekes, King Stanley, *Glouc.*, Clothier; for improvements in the dressing or finishing of woollen and other cloths or fabrics requiring such a process. April 4.—June 4.

70. Joseph Lincoln Roberts, Manchester, *Lanc.*, Merchant; for improvements in looms for weaving. *For. Comm.* April 4.—Oct. 4.

71. Reuben Bull, Adams-st. West, Portman-sq., *Middx.*, Ironmonger; for improvements in chimney-caps to facilitate the discharge of smoke, and to prevent its return. April 15.—Oct. 15.

72. Horatio Nelson Aldrich, Rhode Island, *U. S.*, but now of Cornhill, *Lond.*, Merchant; for improvements in spinning, twisting, doubling, or otherwise preparing cotton, silk, and other fibrous substances. *For. Comm.* April 15.—Oct. 15.

73. Henry Stephens, Charlotte-street, St. Marylebone, *Middx.*, Gent., and Ebenezer Nash, Buross-st., *Middx.*, Tallow-Chandler; for improvements in manufacturing colouring matter, and rendering certain colour or colours applicable to dyeing, staining, and writing. April 18.—Oct. 18.

74. David Napier, York-rd., Lambeth, *Surry*, Engineer; for improvements in letter-press printing. April 18.—Oct. 18.

75. Thomas Hancock, Goswell Mews, Goswell-rd., *Middx.*, Waterproof Cloth Manufacturer; for improvements in the process of rendering cloth and other fabrics partially or entirely impervious to air and water, by means of caoutchouc, or India-rubber. April 18.—Oct. 18.

76. William Crofts, New Radford, *Notts.*, Machine Maker; for improvements in the manufacture of figured or ornamented bobbin-net or twist lace, or other fabrics. April 18.—Oct. 18.

77. Edmund Haworth, Jun., Bolton, *Lanc.*, Gent., for improvements in certain machinery or apparatus, adapted to facilitate the operation of drying calicoes, muslins, linens, or other similar fabrics, (an invention of William William Southworth, Sharples, *Lanc.*, Bleacher;) *term extended five years.*

78. Charles Farina, Clarendon-place, Maida Vale, *Middx.*, Gent.; for an improved process to be used in obtaining fermentable matter from grain, and in manufacturing the same for various purposes. April 18.—Oct. 18.

79. Lemuel Wellman Wright, Manchester, *Lanc.*, Engineer; for improvements in the machinery or apparatus for bleaching or cleansing linens, cottons, or other fabrics goods, or other fibrous substances. April 20.—Oct. 20.

80. William Gratrix, Salford, *Lanc.*, Silk Dyer; for improvements in the process of bleaching or cleansing linens, cottons, and other fibrous substances, and also improvements in the process of discharging colours from the same, either in the raw material or manufactured state. April 22.—Oct. 22.

81. John Gottlieb Ulrich, Red Lion-st., Whitechapel, *Middx.*, Chronometer Maker; for improvements in chronometers. April 22.—Oct. 22.

82. Sir George Cayley, Bart, Brompton, *York.*, for improvements in apparatus for propelling carriages on common roads or railways, part of which improvements may be applied to other useful purposes. April 25.—Oct. 25.

83. James Pinn, Jun., College Green, *Dublin*, Banker, and Thomas Fleming Bergin, Westland-row, *Dublin*, Civil Engineer; for an improved method of propulsion on railways. April 25.—Oct. 25,

84. Miles Berry, Chancery-lane, *Middx.*, Patent-Agent; for improvements in machinery or apparatus for making or manufacturing bricks, tiles, and such other articles. *For. Comm.* April 27.—Oct. 27.

85. Miles Berry, Chancery-lane, *Middx.*, Patent-Agent; for improvements in machinery or apparatus for making or manufacturing horse-shoes. *For. Comm*, April 27.—Oct. 27.

METEOROLOGICAL JOURNAL FOR MARCH, 1837; KEPT AT BLACKHEATH ROAD.

Day of Month		Barom. 9 A.M.	Ther. attch.	Barom. 3 P.M.	Ther. attch.	Thermometer Min.	Thermometer Max.	Daily Temp.	Solar Var.	Rad.	Clouds. A.M.	Clouds. P.M.	Winds A.M.	Winds P.M.	Direction of Wind A.M.	Direction of Wind P.M.	Lunation.	WEATHER, &c.
Wed.	1	30·450	46°	30·458	47°	29°0	37°6	33°3	8°6	28°	9	8	2	2	NE	ENE		Cold cloudy.
Thurs.	2	30·450	45	30·349	46	31·4	40·0	35·7	8·6	30	8	10	1	2	NW	N		Do. Rain and sleet in Evening.
Friday,	3	30·382	44	30·326	46	29·9	41·2	35·5	11·3	27	7	7	2	2	NE	NbE		Do. Do. 6 P.M.
Satur.	4	30·150	46	30·151	47	36·3	43·8	40·1	7·5	34	10	5	3·4	3	N	NNE		High wind, with much cloud.
SUN.	5	30·170	46	30·052	48	32·4	44·2	38·3	11·8	30½	7	8	1	1	wbN	W		Fair. Much cloud, hazy.
Mon.	6	30·042	48	30·086	49	37·2	41·5	39·3	4·3	34	10	10	1	1	NE	N	●	Cloudy and cold.
Tues.	7	30·155	49	30·176	50	34·0	44·6	39·3	10·6	33	7	3	0	1	NEly	Nly		Broken clouds A.M. Clear P.M.
Wed.	8	30·304	48	30·262	49	30·8	45·8	38·3	15·0	28	4	7	1	2	WNW	wbN		Fine; *cumuli* forming into *cum.-stratius.*
Thurs.	9	30·196	50	30·092	51	37·6	49·8	43·4	11·5	35	8	8	3	3	W	W.		Windy; much cloud; appearances of rain.
Friday,	10	29·750	51	29·506	52	38·2	47·7	43·0	9·5	36	5	7	2·3	3	SW	SW		Do. Do. [*nimbus.*
Satur.	11	29·398	51	29·364	53	34·0	45·8	39·9	11·8	31	4	2	2	2	SW	SWbW		Fine *cumuli*, *cirri*, and *cum.-stratus* forming into
SUN.	12	29·480	50	29·549	52	29·1	43·0	36·0	13·9	26	5	8	1	0	sly	wly. E		Showers of rain, snow, and hail.
Mon.	13	29·900	48	30·060	50	33·5	42·6	38·1	9·1	33	10	8	2	2	NbE	NE		Cloudy; a few drops of rain, mixed with sleet.
Tues.	14	30·426	46	30·394	48	30·7	43·8	37·2	13·1	28	7	6	3	3	NE	ENE	☽	Windy; flying clouds. Black stormy Night.
Wed.	15	30·258	45	30·165	46	35·0	39·2	37·1	4·2	33	10	10	2	2	NE	NE		Cloudy and cold. The sun not once seen.
Thurs.	16	30·150	46	30·175	47	35·7	40·0	37·9	4·3	35	10	10	1	1	NE	NE		Do. Drizzling rain, and misty.
Friday,	17	30·356	47	30·378	48	35·5	38·6	37·1	3·1	34	10	10	1	1	NE	NEbE		Do.
Satur.	18	30·310	48	30·250	48	34·2	41·2	37·7	7·0	33	9	10	2	2	ENE	ENE		Do. Fine moonlight Night.
SUN.	19	30·128	46	30·052	48	29·0	40·4	34·7	11·4	27	3	9	2	2	EbN	NNE		Hoar frost; cold wind.
Mon.	20	30·000	44	29·956	46	29·8	37·8	33·8	8·0	27	5	7	2·3	2	N	NE		Squally, with snow-showers. Hard frost in Even.
Tues.	21	29·955	36	29·855	37	23·2	33·8	28·5	10·6	19	7	7	1	1	N	Nbw		Severe frost; snow till Noon. Cloudy Night.
Wed.	22	29·850	39	29·878	43	26·9	38·0	32·4	11·1	25	5	7	1	1	SE	SE		Do. *Cumuli* into *cum.-stratus.* Light snow from 8 to 9.
Thurs.	23	29·812	44	29·790	46	24·6	38·5	31·6	13·9	21	4	4	1	0	SE	SE. E	○	Do. *Cirro-stratus.*
Friday,	24	29·828	42	29·861	44	20·2	36·9	28·5	16·7	16½	1	2	0	1	ENE	Nbw		Intense frost; rime; *cumuli.* Ice an inch thick.
Satur.	25	29·896	43	29·850	45	28·4	39·9	34·2	11·5	27	6	6	2	1	NW	NW		Flimsy flying clouds. Frost at Night till 12. [*bus.*
SUN.	26	29·814	46	29·832	47	30·8	42·1	36·5	11·3	26	5	5	1	2	NW	W		A shower of sleet, 3 A.M.; *cum.* into *cum.-stratus*, *nim-*
Mon.	27	30·082	45	30·120	47	24·0	38·8	31·4	14·8	21	4	3	1	2	WNW	W		Very sharp frost; *cumuli;* snow at Night. [snow.
Tues.	28	30·064	47	29·996	49	31·5	47·8	39·6	16·3	29½	5	5	3	3	W	WSW		Windy; *cirri* in patches; cloudy at Night; thick ice,
Wed.	29	29·802	50	29·772	51	35·9	49·1	42·5	13·2	34	8	10	2	2	wbs	wbs		Showery; much overcast; rainy Night.
Thurs.	30	29·862	50	29·856	50	34·2	45·5	39·8	11·3	33	8	4	0	1	E	NE	☾	Overcast till Noon; fine Afternoon; cloudy Midnight.
Friday,	31	29·999	46	30·006	50	26·1	44·0	35·6	17·9	24	2	3	1	0	NE	N. S		Clear, with hoar frost, with ice.
Mean		30·046	46	30·020	47	31·26	42·01	36·65	10·75									

Bar. Max. 30·500 in. on the 1st.	Ther. Max. 49·1° on the 9th.	Lowest point of Rad. 16½°, on the 24th.
Bar. Min. 29·364 in. „ 11th.	Ther. Min. 20·2° „ 24th.	Rain and snow fallen, 0·66 in.

PREVENTION OF EXPLOSION IN STEAM-BOILERS.

II.

[In the Second Volume of this Magazine* we gave, under the above head, an account of the origin and progress of an inquiry into this momentous subject, begun and carried on, in the United States, by the Franklin Institute of the State of Pennsylvania, with some (uncommunicated) amount of pecuniary assistance from the General Government †.

From the arrangement made by the Institute, and by the Committee appointed by them, three distinct Reports would naturally be produced; viz.:—

1. A General Report of the Committee.
2. A Report of the Experiments confided to the care of a Sub-committee.
3. A Report of the Experiments on the Strength of Materials used in the construction of Steam-boilers, intrusted to a second Sub-committee.

That these will all ultimately be given to the world, we have not the smallest doubt; but certainly not in the above order. No. 2 was the first published. It was presented to the Board of Managers on the 23rd of December, 1835, and given in the five succeeding monthly Numbers of the Journal of the Institute. No. 1 was presented to the same Board on the 21st of September last, and is now in the course of publication in the same Journal. Of No. 3 we yet know nothing.

Our notice, to which we have alluded, contained a cursory examination of the Report No. 2, and we expressed our anxiety for those which were to follow.

On the receipt of the October Number of the Journal, we were gratified to see the commencement of another Report on the subject, though not a little surprised to find it to be the General Report of the Committee, and not the Report on the Strength of Materials, which we supposed (from No. 2 having been published previously to this General Report) would be the course adopted, and which must have been received and deliberated upon, we should have thought, before a General Report could be satisfactorily drawn up.

Owing, we imagine, to the very unfavourable state of the weather, the subsequent Numbers of the *Franklin Journal* did not reach us in their usual order—and at the time of writing this, we are still without the December and January Numbers—so that, from the earnest desire to receive and communicate these interesting and important documents, we became participators in the late general anxiety for arrivals for America.

The offer of a friendly *Savan*, who has recently landed in this country from the United States, relieved us from this state of suspense. Thoroughly acquainted, before he left America, with all the proceedings on this important inquiry, he has been kind enough to place at our disposal an analysis of the General Report. This we have accepted, and are exceedingly grateful for. We shall present it our readers nearly as we received it, but having no time or means to examine its extent or accuracy, we hope our obliging correspondent will not be offended if we imitate some brother-publishers of periodicals in this country, and premise that the customary cautionary formula is understood to be prefixed. It may be observed, that we have said that we present this account of the Report "nearly as we received it," and our reason for so having done we shall give.

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* P. 114.

† Why should not this amount be known? Concealment excites suspicion that it was unworthy of a great nation.

It is pretty generally believed in Europe that the sensibility of our transatlantic friends to foreign criticism is morbidly exquisite. We are disposed to admit, merely from the universality of the opinion, that this may be true to a certain degree; and, admitting this, we cannot be either surprised or offended at the snarl which escapes a writer in the *Journal of the Franklin Institute*, on a perusal of our "cursory remarks;" nor with the *naïf* attempt of the friend who has so timely assisted us, to make us *eat our own words!* Perhaps he has no idea of the difficulty of this species of mastication. We found it impossible, and therefore omitted a sentence or two which had this intention only, and no relation to the General Report.

We did this to avoid a discussion, which might only irritate, and to give unnecessary pain or annoyance is very far from our wishes. The cause of humanity is deeply indebted to the persevering exertions of the *Franklin Institute* on this subject, and we feel great respect for the ingenuity and unwearied application of the members who have conducted the experiments. We therefore, without withdrawing any of the strictures we made in our cursory remarks*, repeat the language we used in the same paper, and congratulate the *Franklin Institute* upon their resolution to inquire into the causes of the dreadful events which are too common in the steam-navigation of the United States, and even in that of Europe. We say again, that "such a philanthropic determination was in perfect harmony with the spirit and conduct of the great man whose name they have adopted as a distinctive appellation; and if they have not hitherto perfectly succeeded in developing every point of this intricate inquiry, they have the distinguished honour of being the first public or private body which has attempted an examination of the subject, by experiments and apparatus on a large scale designed on purpose to bring the more important current opinions and theories to the test of actual proof†."

If this voluntary and honest tribute to the merits of the *Franklin Institute* be undervalued or overlooked, because the same friendly and impartial spirit pointed out a few omissions, and marked some conclusions as hastily drawn, we shall much regret it; and though we do not intend the exhibition of such very inordinate self-esteem on the part of Brother Jonathan shall diminish the warmth of our attachment to him, it may *pretty considerably* augment the purgative effect of any dose we may have, in future, to administer for the removal of a conceit so injurious to his interests, and annoying to his real friends.]

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* Except the allusion to the "whereabouts" of the *Shamrock.* We did assist our *vision by that of* an intelligent friend, as advised; we found it differed from ours, and we *submit cheerfully* to the correction of *the corrector of proof sheets*, whoever he may be.

† See Vol. II. p. 114.

ANALYSIS OF THE GENERAL REPORT OF THE COMMITTEE OF THE FRANKLIN INSTITUTE OF PENNSYLVANIA, ON THE EXPLOSIONS OF STEAM-BOILERS.

WE propose to give only a popular sketch of the experiments, and of the conclusions drawn from them, not, however, with an intention that this sketch should be a substitute for the original reports; on the contrary, we would earnestly recommend the study of these to all, especially to those immediately interested, as containing essential numerical details.

As steam explosions are not all produced by the same immediate cause, they are observed to occur under very different circumstances. To give as much information in relation to these circumstances as possible, the general Committee, as their first operation, addressed circulars to persons practically conversant with the use of steam in various parts of the United States. The answers to these were published in the *Journal of the Franklin Institute*, and thus formed a valuable collection of facts, which directed the Committee to select the most important for experimental investigation. A general view of the facts thus obtained is incorporated in the report.

The different causes of explosion are discussed by the Committee under five heads, which we shall notice in the order adopted by them:—

I. *Explosions from undue Pressure within a Boiler, the Pressure being gradually increased.*

IT has been concluded by some, that ruptures, and not explosions, are produced, particularly in copper boilers, by gradually increasing the pressure, and that these do not partake of the nature of explosions. The reports, however, show that explosions of the most violent kind are produced by steam gradually increased in tension. For experiments on this head two cylindrical boilers were separately used, one of iron $8\frac{1}{2}$ inches in diameter, and 10 inches long, the metal ·02 of an inch thick; the other of copper, $8\frac{1}{2}$ inches in diameter, 12 inches long, and ·05 of an inch in thickness. A self-registering apparatus was attached to mark the maximum pressure. The iron boiler was first used half-filled with water, and heated gradually by a charcoal fire. It at length exploded at the pressure of eleven atmospheres, giving a report resembling that from a small mortar fully charged. The head was thrown to the distance of fifteen feet, the boiler and registering apparatus six feet, the furnace, weighing about forty-five lbs. was overturned, and carried four feet.

The copper boiler was exploded in the same manner, by gradually increasing the temperature. A dense cloud of smoke and flame capped with steam, at the moment, was observed to rise from the furnace, while the stones and combustibles were widely scattered; the boiler was thrown in a single mass, about fifteen feet was rent at each end, and also in an irregular lateral course along the water-line, with the sides bent outwards. The registering apparatus was so much damaged, that the maximum pressure could not be learned; the noise of the explosion resembled that from an eight-inch mortar.

These experiments are deemed conclusive that the most violent explosions can occur by a gradual (and without a sudden) increase of pressure. The reports investigate the cause which may render the apparatus usually employed for preventing the gradual accumulation of steam inoperative in the cases of explosions. Among these are, design on the part of the attendant, loading the safety-valve, in order to produce, on an emergency, an increased power of the engine;—the adhesion of the safety-valve from rust, or some accidental cause. They also consider the form of the valve, and give the preference to the disk over that of the cone,—the latter is more easily tightened when imperfectly fitting the seat, but on that account is also liable to adhesion.

The objection is also discussed, which has been urged against the disk on the principle by Roberts of Manchester, and M. Clement of Paris, that a plate has a tendency to approach, or apparently to be attracted, by a small aperture in a flat surface, through which a powerful current of fluid is passing. The intensity of this effect has been shown, by Hachette and others, to depend on the ratio of the diameter of the disk to that of the aperture, and when the latter is nearly as large as the former, as in the case of the ordinary safety-valve, the effect is almost inappreciable, and may be entirely obviated by a simple contrivance which lessens the action of the weight when the valve is slightly raised.

Two disk-valves were used in the experiments of the Committee, the levers of which were graduated, not by simply calculating the distance from the fulcrum, but by actually weighing the whole apparatus, and thus allowing for friction, &c. The pressure within the boiler was indicated by a gauge, or calculated from the observed temperature of the steam. The action of the safety-valve was not made the object of a separate set of experiments, but a register of its action under the various pressures and temperatures throughout the whole series of investigations was carefully made, and the results deduced from the observations, forty-two in number, show that in no instance was adhesion produced with the disk-valve. And that it always rose with a pressure within the boiler less than that indicated by the load on the lever. The ratio of these quantities with one valve is as 1 to 1·03, and with the other, as 1 to 1·1. To prevent an undue increase of pressure of steam, which is of course attended by an increase of temperature, the public regulations for safety, in the use of the steam-engine in France, required two plates, or plugs, of fusible metal, of suitable dimensions, to be attached to every boiler. These plugs are intended to melt or give way when the heat has reached a certain point; and, in order to prevent their breaking by the pressure of the steam, as they approach the melting point, they are covered, or supported, by a plate of iron or copper perforated with holes. This plan of preventing undue pressure was made the subject of a very elaborate set of experiments by the Committee, the result of which led to some curious conclusions in regard to alloys, and also to results unfavourable to the use of the fusible plates as a means of safety, at least as proposed by the French Academy.

One part of this investigation related to the nature of the alloys themselves, to the proper proportions of the metals, to the effect of impurities in these as found in commerce, to the variable changes which take place in different alloys in passing from the state of perfect fluidity to that of perfect solidity, such as the stationary point of temperature, partial fusion, &c. These cannot be condensed, and would, therefore, require too much space to describe.

Another part of this investigation refers to the operation of the fusible plate when applied to the boiler. For these experiments plates of different alloys, and also of different thicknesses, were used and applied to the boiler by a sliding apparatus, which allowed them to be placed and replaced without difficulty. The important circumstance was soon observed, that the plates gave way at a temperature of the steam very different from that which had been previously determined as the melting-point of the same alloy. To investigate the cause of this phenomenon various experiments were made, and the following curious facts developed.

Before the steam within the boiler had reached the fusing temperature of the alloy, several drops of liquid metal appeared in the perforations of the covering plate; these increased in size, and finally rolled off over the sides of the boiler. In one case carefully studied, in which the metal had failed to give way until the temperature was raised 20° above the original fusing-point of the alloy, the plate was found to have been broken by pressure, and not to have given way by fusion. It had lost its metallic lustre; the fusing-point of the part which had oozed out was also found to be much below the original melting-point of the metal, and also of the remainder of the plate. The original alloy was solid at 254°, and fluid at 275°. The first portions which oozed out were fluid at 223°; the second portions at 235°; and the residuum not until from 312° to 345°, passing by slow gradations from the solid to the liquid state.

A clue being thus afforded to the phenomenon, this method of analysis was imitated with an iron tube, perforated near the lower end, which was closed, and a metallic piston, to which pressure was applied. Finding that various alloys were thus separable by pressure, at temperatures much below the temperature of fusion of the mass, the Committee came to the conclusion, before stated, that the fusible metal used in the ordinary way will not answer the purpose intended.

Although this use of the fusible alloy is condemned, yet the Committee recommend a novel and ingenious application of it to give notice of approaching danger from an increase of temperature within the boiler, either by the presence of metal unduly heated, without being in contact with water, or from the increase of temperature accompanying an increase of tension of ordinary steam. This consists in putting a quantity of the metal into the bottom of an iron tube let into the boiler, so as to be surrounded with the steam. In this tube a stem of metal is placed and soldered, as it were, at its lower end to the bottom of the tube by the fusible metal. The upper end is attached to the shorter arm of a lever, on the other arm of which a weight is placed. When the steam attains a given temperature, the soldering gives way, the stem rises, the weight falls, and thus rings a bell, or opens a stop-cock. By this construction, which is due to the chairman of the Committee, Professor Bache, the metal is not exposed to the pressure of the steam; no large aperture is formed through which the moving force of the engine can escape, and thus give rise, in some cases, to a new source of danger. The apparatus is also immediately repairable, for when the warning is given, and the temperature within has been reduced, the stem temporarily kept down in the tube, becomes fastened by the solidification of the metal, and is again ready for use.

II. *Explosions produced by the presence of unduly-heated metal within a Steam-boiler.*

If parts of a steam-boiler become highly heated,—say to redness,—danger results, from two causes. The tenacity of the metal is diminished, and the heat which it contains, if transferred to water, will suddenly produce an increase of elasticity in the steam.

The repulsion existing between highly-heated metal and water is so great, that the latter of the positions just taken has been doubted, and was submitted to an elaborate examination by the Committee. Their results are given in two subdivisions of the Report; the first subdivision has reference to the fact of the production, or not, of highly-elastic steam, when water is thrown into a red-hot boiler. In the second, the circumstances are examined attending the introduction of water into vessels of copper and iron heated to various temperatures.

The experiments on the first head showed that water injected into a red-hot iron cylinder was rapidly converted into high steam. Different quantities of water thrown in produced steam, which varied in pressure from three to twelve atmospheres, the time required to produce the steam, in the last case, being less than two minutes. The pressure of twelve atmospheres was sufficient to burst a glass-plate inserted in one of the ends of the boiler, thus producing a mimic explosion. Water thrown on the heated metal is fully shown to be the source whence the highly-elastic steam is produced, but the Committee further examined, by direct experiment, an opinion which has been urged by a practical engineer, that water mingled with the hot and unsaturated steam within the boiler is the source of danger.

Steam was formed within a boiler, and then a fire of charcoal applied around the top and sides, so as to communicate heat directly to the steam, until it became highly surcharged. In one experiment of nine, given in the table of results, the steam had a temperature of 533° Fahr., and a pressure of only 6·8 atmospheres; while, if it had been of the full density due to this temperature (*i. e.*, had been saturated steam), it would have had an elasticity of more than sixty atmospheres*.

Water was thrown into this hot and unsaturated steam under different circumstances. The temperature of the water and its quantity were varied, and it was thrown, in spray, through small holes, and also in a body, through one larger aperture. In no case an increase, and generally a decrease of pressure, was found to result from the introduction of the water.

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* *In this case*, by throwing in fourteen fluid ounces of water, the pressure fell *from 6·82* atmospheres to 6·48 atmospheres.

Without following the Committee into the detail of their experiments on the vaporizing power of the metals, at different temperatures, and under different circumstances, and the repulsive principle brought into action, we must be content to indicate the course of experiment, and to give the conclusions.

The vessels used into which to introduce water were portions of spheres of copper and iron, thirteen in number, and of thicknesses varying from ·07 to ·5 of an inch. Their surfaces were varied by polishing, tarnishing, eroding by an acid, &c.

In a first series of experiments, drops of water were thrown upon the metals gradually heated or cooled, and the time of vaporization at different temperatures noticed. In a highly-polished bowl of copper, the most rapid vaporization was at a temperature as low as 292° Fahr., while, on the same surface, less-highly polished, it was 328½° Fahr., and on the same oxidated, and otherwise not clean, it was as high as 348° Fahr.; but the time of vaporizing the same small quantity of water was, in the first-mentioned case three seconds, and in the last, only one-quarter of a second.

In a smooth iron bowl, the temperature of most rapid vaporization is fixed at 334° Fahr., and for the same bowl, when oxidated, at 346½° Fahr.

When the drops of water are repelled from the surface of the heated metal, vaporization appears to go on from the surface of the fluid, which slowly disappears, turning meanwhile about a variable axis, and in opposite directions. Giving a motion to the drop, by letting it fall upon the side of the bowl, instead of on the lowest point, influences the temperature at which this repulsion is fully developed, lowering it on the scale of temperature, when motion is given by dropping upon the sides.

The more practical question, of which a solution was sought by the Committee was, "At what temperature of a metal will water thrown upon it in a limited quantity be most rapidly turned into steam?" This question they endeavoured to solve, by applying, in succession, different quantities of water to bowls of copper and iron heated to the same temperature, and thus deducing a law which might apply to quantities increased as far as the contents of the vessels would permit. Secondly, by varying the mode of communicating heat from a bad conductor and circulator, such as oil, to a good conductor like tin, to eliminate a variable element, viz., the mode of communicating heat.

As the temperature of most rapid vaporization was not reached in many cases, the Committee have supplied this point, by constructing, graphically, curves to represent the rates of vaporization, and estimating from those which indicate the law of the phenomenon, the temperature of most rapid vaporization. Curiously enough, in the cases of the copper bowls, these curves are shown to be ellipses.

The detail of these experiments is given in Vol. II. of the *Magazine of Popular Science*, p. 120.

These experiments show why steam is rapidly produced by throwing water in quantity upon red-hot metal, for the place of contact requires to be reduced in temperature but 200 degrees to reach the temperature of most rapid vaporization; and as the water passes from one portion of the heated metal to another, this effect, no doubt, is partially produced.

At the conclusion of this part of the subject an interesting fact is developed, in regard to the vaporization of alcohol, which, if we mistake not, had previously been noticed in regard to water thrown into a platinum crucible heated to bright redness. It is that alcohol, when introduced into a metallic vessel (copper was used,) heated considerably above the boiling-point of the fluid, does not boil, but evaporates from the surface, and takes a stationary temperature lower than its boiling-point, in proportion as the temperature of the vessel is raised. This phenomenon, however, takes place at temperatures readily measurable, while that with water does not. We agree with the Committee in considering the subject as worthy of minute investigation, which the nature of their experiments did not require or permit.

The great energy of the steam developed when water is thrown upon heated metal, accounts satisfactorily for explosions which have followed such circumstances, but the doubt in regard to this rapid production of steam (and to which we have before alluded) has induced some engineers to embrace a theory which supposes gases to be produced within an unduly-heated boiler from the decomposition of the water by the red-hot metal. If hydrogen were thus produced, and oxygen could be supplied in such a proportion as to unite explosively with it, the red-hot metal would effect their combination. The Committee, in the General Report, remark upon the improbability of the suppositious manner by which oxygen is supplied, but they make the fact of the production, or non-production, of hydrogen the subject of direct experiment. Water was thrown upon the bottom of a boiler heated to orange redness, and the elastic fluids formed collected over water, which, condensing the steam, would allow any gas to remain over it. No hydrogen was produced, the metal of the boiler being clean but not bright, that is, having a scale of oxide upon it. Gas in considerable quantities was collected, but it was nitrogen, with a little oxygen, from air that leaked into the boiler during the heating, which took place, of course, without the presence of water.

In some of the trials carburetted hydrogen was collected, but this was traced satisfactorily to the oil in the putty, to the cloth, &c., of the packing of the arm-hole, giving access to the interior of the boiler, and ceased to be produced when these were charred.

Having investigated the source of danger from heated metal, the Committee go on to consider the circumstances which may lead to its production. These are examined as follows:—

1. By a deficient supply of water.
2. By deposits from the water used to supply the boiler, or from other sources.
3. In arrangement of contiguous and communicating boilers on board of steam-boats, by the inclination to the horizon of the deck of the boat.

1. In considering this head, it is shown that, to lower the level of the water one inch in a cylindrical boiler, with an interior furnace or flue bearing the most common proportions in diameter to that of the boiler, will require but five minutes, supposing no supply of water to the boiler. It seems further, that there are well-authenticated instances in which this cause was operative *. Some of the means by which the water may be thrown upon the metal are ordinary, such as the removal of an obstruction which may have occurred in the supply-pipe from the forcing-pump to the boiler, the supply of water by a hand-pump, &c. There is, however, one very curious means which has been assigned, and which is examined by the Committee. When water, boiling under pressure, is suddenly relieved from this pressure, it foams up, and the foam may be thrown upon heated metal supposed to exist within the boiler. This supposition seems actually to be required by the phenomena of certain explosions; as, for instance, by that at Polgooth, where two connected boilers exploded in succession; so that just before the explosion of the second, a large escape had been supplied for the steam within it, by the rupture of the pipe which connected it with the first.

The fact of the foaming of water when heated, and suddenly relieved from pressure, was made the subject of experiments, detailed in the first part of the Report. They were made in a small glass boiler, and also in a metallic boiler provided at the ends with glass plates, so as to give a view of the interior.

The phenomenon is important in two points of view: first, in its effect upon the apparatus which is used to determine the level of the water within a boiler; and second, in reference to the supposition to which we have referred above. In both these points of view it was investigated by the Committee. In the first, its extent was shown to be such as to produce false indications in the gauge-cocks, commonly used to determine the level of the water within a boiler. This is particularly the case in small high-pressure boilers, where the pressure is great, and the volume of the steam-chamber small. It affects also, of course, the float used for a purpose similar to that of the gauge-cocks. The Committee prefer to either of these apparatus for determining the level of the water, a glass tube connected with the boiler. It seems that, in applying it to high-pressure boilers, white glass corrodes and becomes nearly opake; a defect which may be remedied by using green glass, which contains less alkali than the white. A drawing, upon a tolerably large scale, showing the various parts of the tube water-gauge recommended, is given in the General Report. It does not differ essentially from that in use in British locomotive engines, and figured in the work of the Chev. Du Pambour.

It seems to be a difficult point to determine whether the foaming produced by making an opening into a boiler actually leads to the production of more, or of less, steam than escapes by the opening. In the experiments of MM. Arago and Dulong, at Paris, and in those of the Committee of the Franklin Institute, making an opening was always

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* Not less than five are referred to, as given in the communications to the Committee.

attended with a decrease in the elastic force of the steam within the boiler; while in those of MM. Tabareau and Rey, at Lyons, an increase of elasticity was observed on making a similar opening. The Committee explain this discrepancy as the effect of the different circumstances of the experiments, in regard to heated surface. While in the Paris experiments the boiler was, probably, not unduly heated in any part, in those of the Committee a band merely, just above the water-line, was thus heated, and in those at Lyons the whole boiler was surrounded by a charcoal fire. In extraordinary cases, they admit the foaming produced by an opening as a cause of explosion, and recommend in every case the avoidance of such a source of risk.

In the General Report we find next considered the methods proposed for avoiding a deficiency of water within a boiler. We have only room to give the results. (1.) Self-regulating apparatus for feeding boilers, especially high-pressure boilers, should not be substituted for the care of an engineer. (2.) The glass gauge-tube is the best means hitherto proposed for determining the level of the water within a boiler. (3.) The danger from undue heating of parts of a boiler may be avoided by the use of fusible metal, under the conditions to which we have heretofore referred at length.

We proceed to the discussion of the second cause assigned as likely to produce undue heating of parts of a boiler; viz., by deposits of sedimentary matter.

These, it seems, are especially rapid in forming in the boilers fed by the calcareous and muddy waters of the rivers in the western parts of the United States. The results communicated from different quarters in regard to the nature of these sediments, and to the circumstances under which they are deposited, are valuable.

After examining the methods proposed for preventing the accumulation of sediment, and its hardening upon the bottom of the boiler, such as "blowing off" the water by a cock placed near the bottom of the boiler, farinaceous substances, chemical agents, filtration, &c., the Committee conclude that no substitute has yet been found for thoroughly cleansing the boilers from time to time. The periods at which this cleansing must take place will vary with the nature of the water used to supply the boiler.

We next pass to the heating caused by a change of level in the deck of a steam-boat, having connected boilers. It appears by the General Report that, on the western waters of the United States, small cylindrical boilers are commonly used. They lie parallel to each other, and to the longitudinal axis of the boat, and are connected both from the water and steam-chambers. When the deck of the vessel is level, the water in each of these boilers occupies the same portion of the cylinder. But if the vessel inclines to either side, the boiler on the higher side has less water in it than that on the lower, and thus a portion of the metal in the former is exposed to the direct action of the fire without being covered by water, and may become highly heated. When the vessel returns to its level, water will be thrown into the boiler thus *heated*, in which steam will be rapidly produced. Instances of explosion are given in the Report, which have, probably, been thus caused; and, after reviewing the remedies proposed, the Committee recommend that the boilers be separately supplied with water, or that two, at most, be connected.

III.—*Explosion arising from defects in the construction of the Boiler or its appendages.*

In this division of the subject, the Report of the General Committee consider the forms, materials, and manufactory. The old wagon-boiler of Watt should be used when very low steam is employed. Observation has shown that boilers with flues, or interior furnaces, commonly give way by the yielding of the flues, and by the blowing off the heads. Of the different kinds of flues, those which pass directly through both heads are considered the least dangerous. Connected boilers on board steam-boats are liable to a source of explosion, which has already been mentioned,—the careening of the vessel; the Committee are of opinion that they cannot be used with safety, and ought to be laid aside, or separated at least into pairs. The danger arising from water coming suddenly in contact with metal highly heated has already been shown; any boiler, therefore, which has a part exposed to the heat, without being covered with water, or is liable to such an exposure, is essentially bad. The L-flue boiler is of this kind, in which a part of the flue or chimney passes vertically through the space occupied by the steam. The flue thus situated becomes unduly heated, and has thus produced explosions by a collapse of the flues. The discontinuation of the use of this form of flue is recommended, also all projecting tubes down into the fire are considered defective; the weakness arising from irregularity of form should always be kept in view.

The materials in common use for steam-boilers in the United States are wrought-iron and copper; cast-iron has been generally abandoned.

A very extensive set of experiments have been made by a second Sub-committee on the relative strength, at various temperatures, of all the different kinds of iron and copper used in the construction of steam-boilers in the United States; but the labours of this Committee have not yet been published.

The strength of a boiler may undoubtedly be lessened by a constant and unequal pressure, so as to give way under a force less than it had once borne. Boilers are liable to be corroded by salt-water; copper, not being so rapidly acted on as iron, should be preferred in situations exposed to this action.

IV.—*Explosions resulting from the Carelessness or Ignorance of those intrusted with the management of the Steam-engine.*

These, no doubt, go hand in hand, and there is little hope of remedying the former, except by enlightening the latter. The Committee remark very judiciously, that the law should interpose to prevent the loss of life which is attendant on these causes. A steam-engineer should be a person having at least a general knowledge of the agent which he is put to control, and he should exercise his responsible duties under suitable penalties against neglect.

V.—*Examination of particular cases of Collapse of a Boiler or its flues, by rarefaction within.*

These are stated to be of rare occurrence. The first being provided against by the air-valve opening inwards in low-pressure boilers, and high-pressure ones not being liable to it. A curious case, which occurred at the Mold Mines, was ably described by Mr. John Taylor, and serve as an instance for the remarks of the Committee. In this case, an explosion of gases from coal took place in the interior flue of a boiler, and was immediately succeeded by a collapse of the flue. It suggests precautions where bituminous coal is used, not to close a damper too soon after the introduction of fuel.

The Report, in the examination of which we have been engaged, concludes with a consideration of the question, whether it is possible to provide protection against explosions on board of steam-boats when they occur.

The means which have been proposed are, placing the passengers in a different boat from that carrying the engine, placing the boilers on the guards of the boat, and separating them from the parts occupied by the passengers by a suitable bulwark.

The Committee remark, that the former of these plans has been tried in the United States, but was laid aside for want of patronage, on account of the moderate speed which is attendant on this method.

The latter they consider as but partially carried into effect, by placing the boilers on the deck and upon the guards of the boats, as is practised in most of the boats in the Atlantic States of the Union.

We agree entirely with the Committee in considering that attention should be directed by law, rather to make the boiler safe and preventing explosions, than to bulwarks between the boiler and passengers.

It appears to us extraordinary, that in the United States, where steam-navigation is so extensively resorted to, no laws for safety have yet been enacted: and we conclude, by heartily wishing the Committee success in their endeavours to procure the passing of a law upon the subject.

ON DIETETIC CHEMISTRY.

IV.

A very interesting department of organic chemistry, closely connected with the present subject, is that which relates to the *transmutations* of which vegetable substances are susceptible, and of which, the change of starch into gum and into sugar, and of wood into those substances, are instances; and it will be obvious, by reference to what has already been stated, that, in regard to *lignin*, the theory of the change is expressed, by the abstraction of carbon, or the addition of water. We cannot artificially *convert* sugar into wood, because we have not the means of causing it *chemically* to combine with the additional proportion of carbon requisite for that change; but we have the power of abstracting the carbon from wood, or of adding the elements of water to it, and, consequently, of converting it successively into vinegar, gum, starch, and sugar.

Our subject now leads us to the extraordinary conversion of sugar into *alcohol;* to the consideration, that is, of the theory and products of fermentation. To this process sugar is apparently essential; but, inasmuch as we can change wood into starch and into gum, and these into sugar; so wood, starch, and gum, may become indirect sources of alcohol; and that this is the case, is shown in the production of those fermented liquors which are obtained from *malt*, of which the various kinds of beer and spirit furnish examples: and here, attention must again be directed to those remarkable changes which seeds undergo during germination, and fruits in ripening, which have already been briefly adverted to on a former occasion.

The process of *malting* consists in suffering barley, or other grain, to *germinate* to a certain extent; that is, to let the seed grow, or vegetate, up to that point at which it contains its maximum of sugar: if allowed to continue growing, the sugar at first formed is afterwards consumed by the young plant; so that, at the proper period, vegetation is checked by heat, and then the barley is said to be *malted*. Now, the chemical analysis of malt, as opposed to that of barley, shows what has happened; and as sugar is essential to the production of alcohol, that is, to *vinous fermentation*, we see how the grain is, as it were, prepared for those further changes by the maltster*.

The fact is, that when once sugar is present, whether formed by germination, as in malt, or merely added to the ground and unmalted grain, it equally has the power of determining the conversion of a further quantity of starch into sugar; so that it is only necessary to *begin* the change, and then, in fermentation, as in germination, a further and successive conversion continues; hence it is, that a little malt, added to a large quantity of unmalted grain, will adapt the whole mixture to the purposes of fermentation, as we see in the preparation of the *wash* for the use of distillers, and in other cases: however, not to complicate the subject with too many illustrations, and, especially, to limit it in its application to dietetic chemistry, we must here confine ourselves to a few observations on beer and wine.

In making *beer* the malt is ground into a coarse powder, and infused and agitated in hot (but not boiling) water. The resulting infusion is termed *wort*, and its strength or density is adjusted by an instrument which shows its specific gravity, and which is erroneously termed a *saccharometer*. The wort is then boiled with *hops*, which, by their aromatic bitterness, conjoined, perhaps, to some slightly narcotic power,

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* Comparative composition of ripe barley in the unmalted and malted state:

BARLEY.		MALT.	
Gum	50	Gum	140
Sugar	40	Sugar	160
Gluten	30	Gluten	10
Starch	880	Starch	690
	1000		1000

give flavour and odour to the liquor, cover its sweetness, and augment its soporific powers; it is then run off, first into coolers, where it is as quickly as possible reduced to a comparatively low temperature (about 50°), and then into the fermenting-vat, having been previously mixed with a little yeast, by which its fermentation is accelerated, and the chance of its acquiring acidity greatly diminished. It is in the fermenting-vat that the principal chemical changes of the liquor are perfected, consisting essentially in the disappearance of sugar, and the production of *alcohol;* so that the liquor gradually becomes less sweet, and more spirituous and intoxicating.

These changes are independent of any external agent, except a due temperature; the presence of air is not only unnecessary, but its access is always rather prejudicial than otherwise; and the leading chemical result is the formation and copious evolution of carbonic acid gas, together with the increased temperature of the liquid. The nature of carbonic acid has already been adverted to; it is a compound of one atom of carbon and two atoms of oxygen; hence, if there be no error in our conclusions, sugar is converted into alcohol by the loss of carbon and oxygen, in such relative proportions as to constitute carbonic acid; hence we should infer a great residuary excess of *hydrogen* in alcohol: now such theoretical inference is beautifully substantiated by experiment, for we find that sugar, *minus* carbonic acid, forms alcohol; and the elements of alcohol, *plus* carbonic acid, are those of sugar. This may be expressed numerically, by stating, that 45 parts of sugar contain the elements of 22 parts of carbonic acid, and of 23 parts of alcohol; or, in other words, sugar is resolvable into nearly equal weights of alcohol and of carbonic acid; and, in the process of fermentation, the former is chiefly retained in the liquor, whilst the greater part of the latter escapes in the form of gas.

In alcohol, therefore, we have the same elements as in sugar, but in different proportions; the alcohol being especially characterised by its excess of hydrogen; hence, when burned, it yields, along with carbonic acid, an enormous quantity of water*; and when decomposed by the action of sulphuric acid, instead of yielding carbon and water, it affords *ether* and water, or *hydrocarbon* and water. As, therefore, we have formerly represented *sugar* as a compound of carbon and water, so alcohol may be regarded as a compound of olefiant gas and water; or, in other words, sugar is a *hydrate of carbon*, and alcohol a *hydrate of hydrocarbon.*

What has been said of fermentation, as applied to the facture of beer, applies equally to all other cases, namely, to the production of spirit or alcohol from all kinds of grain, from the juice of the sugar-cane, from potatoes, beet-root, and blackberries; but there are a few important features in the case of the production of *wine*, more immediately applicable to the subject of these papers.

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* Alcohol is composed of—

Carbon	522		Water	393
Hydrogen	130	or	Hydrocarbon	607
Oxygen	348			
	1000			1000

The recently-expressed juice of the grape, or *must*, as it is technically called, is distinguished by the power of fermenting into perfect wine, without the help of yeast, or other additions, or *ferments*, as they are usually called, which are commonly essential to the perfection of the process in other cases of vinous fermentation. The apparent cause of this peculiarity is, that it contains a portion of a species of *gluten*, or animo-vegetable matter, together with sugar, gum, bitartrate of potassa, and tartrate of lime; and, accordingly, when merely expressed in the usual way, and submitted to a due temperature, it ferments with precisely the same phenomena as those of a mixture of yeast and wort.

This being the case, the question naturally arose, why fermentation never ensues in the grape itself, which, if left upon the vine, or even after having been gathered, shrinks, and dries up into raisins, without showing the slightest symptom of vinous tendency? Fabroni accounted for this by supposing, that in the grape, the glutinous portion, or ferment, was contained in distinct vesicles, which were not ruptured till the fruit was pressed, and hence, though present, remained inactive as a ferment. But Gay-Lussac found, that when grapes were expressed out of the contact of air, and when all access of oxygen to the juice was carefully avoided, no fermentation ensued: it would therefore seem that the contact of oxygen was essential to give the juice of the grape its incipient tendency to ferment; and it is curious how completely the air must be excluded by the husk or membranes in which the juice is contained, and which, though delicate, and admitting of transpiration, effectively prevent the access or absorption of oxygen from without, and preserve the integrity of the grape. When the juice has only been in momentary contact with the air, it absorbs a sufficiency of oxygen to enable it perfectly to ferment.

The *colour* of wine depends, with very few exceptions, upon that of the husk of the grape, and is either purple, red, or yellow. The pulp of the grape is very seldom tinted; nor is the colouring matter of the husk soluble, to any extent, in water, or in the unfermented juice; but it acquires solubility in proportion as alcohol is formed. Both red and white wines are, however, often artificially coloured; the greater part of red champagne is tinged with cochineal, and brown sherry is coloured with burnt sugar. A peculiar extractive matter is contained in some wine, which deepens its tint when exposed to air; and traces of protoxide of iron are found in others, which, on similar exposure, acquire a blue or blackish hue.

The principal components of genuine wine are, water, alcohol, colouring-matter, tartaric acid, bitartrate of potassa, or tartar, and aroma. The quantity of alcohol is very various; port, sherry, and madeira, contain, upon an average, 20 per cent. of alcohol, of the specific gravity of ·825 at 60°. The average of claret is about 15 per cent., and of hock 12 per cent[*]. Raisin, and other wines made in this country, are often much stronger than the highest average of port, in consequence of the quantity of saccharine matter, or of added sugar, which is suffered to ferment into alcohol.

Wines undergo various changes in cask and in bottle, depositing

[*] See the table of wines in Brande's *Manual of Chemistry*, p. 1068; and in Dr. Henderson's *Treatise on Wine*.

more or less tartar, and, in many instances, acquiring the peculiar etherea *bouquet*, which constitutes their chief excellence.

In reference to the dietetic use of spirituous liquors, it is necessary to remark, that a strong line must be drawn between those which have passed the still, and which may be considered as containing *free* alcohol, and those which have not been subjected to distillation, such as beer, and genuine wine. The former, when indulged in in excess, are infinitely poisonous in comparison with the latter; though, in excess, both are bad enough; and we unfortunately abound in exemplars. The picture of the professed gin-drinker is too appalling to be set before the reader in its details; and the horrors which any one may witness who chooses, for a short hour, in the busy time of day, to visit a gin-shop, are such as to degrade our national character.

Which are the worst, those who legislatively sanction these sinks of infamy, open on all days, and at all hours; those who immediately profit by them; or those who are their willing victims, I will not pretend to determine: all are equally concerned, passively or actively, in the miseries they engender, and all must, one day, account for these horrible sacrifices at the shrine of iniquity. The opposite side of the picture is, fortunately, more favourable; and drunkenness, amongst persons of character and education, is considered, as it ought to be, at once sinful and degrading. The consequence has been, increased longevity, and the disappearance, among the upper grades of society, of a host of distempers, that followed in the train of inebriety.

In small quantity, wine is exhilarating and tonic, and, as such, no substantial objection can be urged against its moderate use; nor can we coincide with those who maintain that it should exclusively be used as a medicine, who maintain that it is either *physic* or *poison*, and who ask, of what benefit its *temperate* use is to a healthful individual, who requires no additional excitement, either of mental or corporeal energy? We might apply the same method of reasoning to all our luxuries and condiments. Even salt itself might, upon such principle, be rejected.

The *dietetical properties* of wine depend partly upon the time at which it is taken, and partly on the nature and quality of the wine itself: the former inquiry we must, for the present, waive, and conclude with a few words respecting the qualities of our ordinary wines.

In this country, notwithstanding the liberal views which have lately unfettered many of our commercial regulations, the use of the stronger wines of Spain and Portugal predominates over the more delicate produce of France and the Rhine; and port, sherry, and madeira, maintain their pre-eminence.

In its new and unadulterated state, port is rough, strong, and slightly sweet; but when duly kept in bottle, it deposits most of its astringent extractive, and loses sweetness, acquiring an improved flavour, and retaining its strength. Various proportions of brandy are almost always added to it before it comes to this country, to which much of its heat and stimulating effect must be ascribed. If too long kept in bottle, nearly all its colouring and astringent matter are deposited; it loses flavour, and becomes less agreeable both to the stomach and the palate. *Good port* wine, duly kept, is, when taken in moderation, one of the most wholesome of vinous liquors; it strengthens the muscular system, assists the digestive powers, accelerates the circulation, exhilarates the spirits, and sharpens the mental energies: in *excess*, it is, perhaps, the most mischievous of the wines, and most likely to produce those permanent derangements of the digestive organs, that obtuse state of the faculties of the mind, and those obstinate organic affections, which follow the habitual use of distilled spirits.

Madeira, as a stimulant, equals port, and, when in fine condition, may truly be called a generous wine; unfortunately it is rarely to be procured, and as it is generally more acid than either port or sherry, it is, consequently, not so well adapted to stomachs inclined to dyspeptic acidity. Sherry, of a due age, and in good condition, is a fine, perfect, and wholesome wine, free from excess of acid, and possessing a dry aromatic flavour and fragrancy; but, as procured in the ordinary market, it is of fluctuating and anomalous quality, often destitute of all aroma, and tasting of little else than alcohol and water. Among the French wines, which, in delicacy of flavour and care in manufacture, exceed all others, *burgundy*, and the various branches of that family, are peculiarly heating and soporific, especially when new. Burgundy is not less celebrated for the exquisite delicacy of its flavour and odour, than for the uncertainty with which it retains them. Sometimes it preserves its excellence unimpaired for many years; at others, it becomes imperfect in a few months. Any sudden change of temperature is very inimical to it; hence it should be kept in a cool, but not cold cellar, and should be transported at a temperate time of year.

The wines of Bourdeaux are distinguished by a delicacy of flavour and bouquet, and by a more perceptible combination of the acid with the vinous flavour (though quite independent of acescency,) than is perceived in most other genuine vinous liquors; they are less heating than most other wines, and agree well when taken in moderation: in excess, they excite acidity and indigestion, apparently rather from quantity than quality. But the *clarets* of our merchants are often very substantial wines, being compounded in various ways for the English market: they are thus often mixed with hermitage, and flavoured with raspberry brandy; and, if procured through doubtful channels, are too frequently acescent, and apparently composed of some claret, mixed with faded port, and other spoiled wines. *Hermitage*, especially *red*, *côte-roti*, *Rousillon*, and a few other wines of rarer occurrence, occupy places intermediate between port and claret, as to strength, and often also as to flavour. These wines require age, and rousillon, if originally of fine quality, is not in perfection unless it has been ten or twelve years in bottle. *Champagne wines* admit of division into two classes; the sweet and sparkling, and the dry and still. These differences arise partly from the mode of managing the fermentation and bottling of the wine, and partly from circumstances of the growth and situation of the vines; the sunny side of the hill yielding fruit fit for the production of the sweet wine, and the opposite aspect affording grapes calculated for the manufacture of a strong but dry wine. The effervescing varieties of champagne, if not taken in excess, are the most speedily exhilarating of all wines, and soon produce a transient approach to intoxication: in excess, they are more than ordinarily pernicious. In gouty habits, champagne, even in moderation, is more apt than other wines to create painful sensations and cramp; yet there are gouty persons who indulge in it with, at least, *temporary* impunity. *Still champagne* is often a strong and heating wine, and very deceitful in these respects to the palate. When of superior quality, it has the singular aromatic flavour of champagne in an eminent degree; a flavour which also exists, but is concealed by carbonic acid, in the sparkling wines. The latter, therefore, should not be drunk till the active effervescence is over, by those who would relish this characteristic quality. The prevalent notion that a glass of champagne cannot be too quickly swallowed, is erroneous; and it is no bad test of the quality of champagne, to leave it exposed for some hours in a wine-glass, when, if originally of the higher order, it will be found to have lost its carbonic acid, but entirely to retain its body and flavour. The coloured champagnes are usually tinted with cochineal, and are often very inferior*.

The class of *sweet wines*, or *vins de liqueur*, as the French term them, require little notice in this place, being rarely taken in quantity exceeding one, or at the utmost two, small glasses. Many of them are potent, aromatic, and cordial; and they are generally more agreeable to the palate than the stomach, with which they eminently disagree, if indulged in beyond the usually prescribed quantity.

A great variety of wines are made in Italy, and in the more southern parts of Europe, which, however, are of rare occurrence, and chiefly found at the tables of the curious. Of these some few are of excellent quality, but in general they are carelessly made, and, from inattention to cleanliness and to the state of the fruit, they are of inferior or even disagreeable flavour. With the exception of *constantia*, the wines imported from the Cape of Good Hope are also defective in the most essential qualities of good wine.

Among the home-made wines, a few are drinkable, and many might be considerably improved, but the necessity of selecting perfect and healthy fruit, and the extreme care and cleanliness requisite in all steps of the manufacture, are here so little attended to, as, with very few exceptions, to render all these products quite unworthy of comparison with those previously mentioned. They commonly contain a large quantity of unfermented sugar, or they have become *pricked* in consequence of the production of a little vinegar, and hence are extremely apt to disorder the stomach.

It may not be out of place here to remark, that the acidity of stomach and other symptoms of indigestion which follow occasional indulgence in wine, may, to a great extent, be prevented by a dose of magnesia at bed-time, which saturates the acid in the stomach, and allays the febrile action.

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* The *Rhine wines*, including hock, moselle, &c., are for the most part moderately alcoholic, and of extremely refined flavours: the average strength of old hock is about 12 *per cent.* of alcohol. These wines are unobjectionable where they agree, which is the case generally, when they are taken alone, and in moderation.

A POPULAR COURSE OF ASTRONOMY.

No. XII.

THE INCLINATIONS OF THE PLANES OF THE ORBITS OF THE PLANETS.—KEPLER'S LAWS.

THE *apparent* motions of the planets have been described to be the same as though they revolved, each uniformly in a circle, round the central sun; and the sun, together with them, were carried perpetually in a circular orbit round the earth; moreover, all these circular orbits have been spoken of as described in the same plane in which the apparent motion of the sun takes place.

Their motions have not thus been *accurately* described. Although, to a first examination, it might appear that their orbits about the sun were circular, and their motions uniform; yet, on a closer inspection, it would be manifest that this mode of describing them requires considerable modification to make it accurately agree with observation. The apparent paths are not, in fact, the same as they would be if each planet moved in a circle round the sun, but in an ellipse; and, moreover, they are not the same as though, together with the sun, they were swept continually in a circle about the earth, but in an ellipse, having the earth in one of its foci; so that, when we come to correct the apparent and complicated system of the universe, by communicating to all the bodies which compose it, the apparent motion of the sun, in an opposite direction, and thus pass to the true hypothesis,—of one central sun at rest, and a system of planets, of which our earth is one, revolving all in the same direction round it,—we shall obtain for the orbit of each, not a circle, but an ellipse, having the sun in its focus.

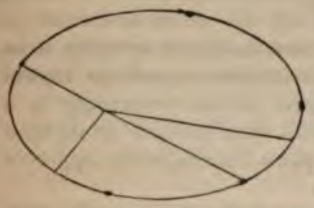
Again, instead of supposing them all to revolve in orbits whose planes coincide with the plane of the sun's apparent revolution, or the ecliptic, we shall find, on closer inspection, that they all, more or less, deviate from it. There is not, however, one of the seven greater planets, the inclination of the plane of whose orbit to that of the earth exceeds 7°, and only one whose inclination exceeds half this quantity.

The inclinations of the planets are at follows:—

Mercury	7°	0′	9″	Ceres	10°	37′	26″
Venus	3	23	28	Pallas	34	34	55
Mars	1	51	6	Jupiter	1	18	51
Vesta	7	8	9	Saturn	2	29	35
Juno	13	4	9	Uranus	0	46	28

In all these orbits there is a slight secular variation, of which that of Jupiter is the greatest, being 22″·6, and that of Mars the least, being only 0″·1523.

That remarkable law by which the motion of the earth in its elliptic orbit is governed, which law is called that of the equal description of areas, has been before somewhat fully explained. Now, precisely the same law is found to obtain in respect to the motions of the other planets in their orbits. The area swept over by a line drawn from the sun to any one of them in a given time, say a month, is always the same, through whatever part of its orbit the motion of the planet may during that month take place; although this line varies in length daily, yet, whether it be longer or shorter, does the planet so regulate its motions, as always to cause it to sweep over the same area in the same time. This law, then, of the equal description of areas, is common to all the planets, and may well be supposed, as it was when first discovered, to result from some principle of motion common to all. The discovery of the principle of motion whence it resulted, was the first invention of Newton; the proof of it is the first proposition of his Principia: it was his first step of approach to the theory of the universe. It results entirely from the fact that the motion of each planet sprung from one original impulse, of which the whole effect remains, combined with the continual attraction of a single centre of force. It has nothing to do with the law according to which that attraction is exerted.



Not only, however, does there exist a relation between the motions of the same planet in different parts of its orbit, but the motions of the different planets in their several orbits are further subject to a common law or relation, which is this, that the times of their complete revolutions, being squared, will be found to have precisely the same ratio to one another that the major axes of the ellipses in which they revolve have when cubed.

This, again, is a law deducible from the same principles, provided that the particular law which governs the attractive force of the sun, is that of the inverse square, as it is termed; or, in other words, provided that it is such as to be equal to a certain quantity, the same at every distance, divided by the square of the distance. Since, then, this relation of the periodic times is a necessary consequence of that particular law of attraction, and that it could not obtain if the law of attraction were any other, it follows, that the law of attraction *is* that of the inverse squares. The nature of this law of attraction may, perhaps, be better understood thus; if we conceive a number of equal particles to be situated at different distances, then will the attraction of the sun upon them be such, that, each attraction being multiplied by the square of the corresponding distance, all the products will be the same.

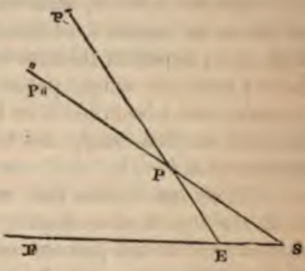
The laws of which we have spoken are called those of Kepler; they were discovered by him before the theory of gravitation had been developed by Newton: they are the experimental, or, rather, the observed facts, on which the whole of that theory is based, and without which it might probably have been imagined, but could not have been demonstrated.

But the discovery of these laws necessarily supposes an accurate knowledge of the actual and real motions of each planet in its orbit, continued from day to day, and through a long period. Now it has been *shown that* their actual motions are prodigiously different from their *apparent motions*. These last are of the most complicated kind; each planet moves apparently in an orbit whose appearance is that of a scroll, looped or zigzag, and although this curve is governed, indeed, by a geometrical law, being of the nature of an hypocycloid, or, rather, an hypo-ellipsoid, yet is that law of so complicated a kind, that the separation of the true from the apparent motion of a planet by means of it, would seem to be nearly an impossibility.

This is not, however, the method of ascertaining the true motion of a planet about the sun, or as seen from the sun, which is actually in use among astronomers. That method is but an approximation; it is, nevertheless, attended with such difficulties, as to place it, perhaps, beyond the reach of a popular explanation.

THE GEOCENTRIC AND HELIOCENTRIC PLACES OF THE PLANETS.

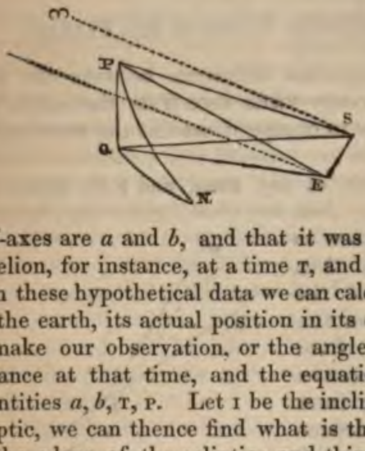
IN the first place, there is an important difference between the position which a planet appears to occupy when seen from the earth, and the position which it would appear to occupy if seen at the same instant from the sun.



Let S be the sun, E the earth, at any time, and P the position of a planet, at the same instant. Join E P, and suppose it to be produced to the region of the fixed stars, then will P be seen in the sphere of the heavens at P' by an observer on the earth at E. But an observer from the sun would see it in the direction of the line S P, at P'', differing from P' by a space in the heavens dependent upon the magnitude of the angle, $P' P P''$. P' is called the geocentric place of the planet, and P'' its heliocentric place. The accurate determination of the heliocentric from the geocentric place, is one of the most difficult problems in astronomy. Could we but determine, at any instant, the actual distance of the planet from the earth, this difficulty would, however, in a great measure be removed. If we knew the side, E P, of the triangle, S P E, knowing its side, E S, which is the distance of the earth from the sun, and observing the angle, S E P, the angular elongation of the planet from the sun, we should at once, by the known methods of trigonometry, be able to determine the angle, F S P.

If the planet moved in the plane in which the earth moves, this angle would at once fix its position; for, knowing the position of the earth, we should have only to measure off this angle from it on the ecliptic, and at once fix the position of the planet. But no planet moves exactly in the same plane as the earth. The line E P joining any position of the earth and planet is not, therefore, in the plane of the ecliptic, and the angle F S P is not in that plane, except at the time when the planet is in one of the points (called nodes) in which its orbit intersects the plane of the ecliptic. Moreover, the actual determination of the distance E P of the planet (by method of parallax), is by no means an easy operation, perhaps, indeed, a scarcely practicable one.

The method most readily practised, is not that of a direct deduction of the heliocentric from the geocentric, but a method according to which an hypothesis is made with respect to the planet's real motion and heliocentric place, and thence its geocentric place deduced. This result being then compared with actual observation (which is, of course, geocentric,) a sufficient number of times, presents the means of determining the hypothetical quantities introduced in the calculation, of ascertaining whether the law of the heliocentric motion is really that which has been supposed, and of fixing all the elements of the orbit.



Let P be supposed to be the place of the planet, E that of the earth, and S that of the sun; also, let S Q E be the plane of the ecliptic, and P Q a perpendicular upon it from P; join Q E and Q S. And let it be supposed that the planet describes an ellipse about the sun, whose half-axes are a and b, and that it was in a known part of this orbit, its aphelion, for instance, at a time T, and has for its periodic time P. Now, from these hypothetical data we can calculate, precisely in the same way as for the earth, its actual position in its orbit, at any time when we propose to make our observation, or the angle which it makes with its aphelion distance at that time, and the equation expressing this will involve the quantities a, b, T, P. Let I be the inclination of its plane to that of the ecliptic, we can thence find what is the above-mentioned angle, reduced to the plane of the ecliptic; and this is the difference of the longitude of the aphelion and planet; and, moreover, supposing L to be the longitude of the aphelion, we may from thence at once find, by addition or subtraction, the actual heliocentric longitude of the planet, in terms of a, b, T, I, L, which is, if ♈ represent the point Aries, equal to the angle ♈ S Q. Moreover, knowing the angle ♈ E S, which is the sun's longitude, and, therefore, its supplement ♈ S E, and knowing ♈ S Q, we know the angle E S Q; also from the hypothesis of an elliptical motion in a known orbit, we can find the radius vector S P of the planet at P; and if we suppose N to be the longitude of the node, having already found the heliocentric longitude of the planet, we can find at once the difference of longitude, N Q, between the planet and its node, and thence, from the supposed inclination, P N Q, we can find P Q. In the right-angled triangle, P S Q, we know, then, S P and Q P, and thence we know S Q; and in the triangle S E Q, knowing S E and S Q, and the angle E S Q, we know the angle S E Q, and the distance E Q, called the curtate distance of the planet. And knowing E Q and P Q, we can find in the right-angled triangle P Q E the angle P E Q. Thus, then, we find the angles P E Q and S E Q in terms of a, b, T, I, N, L.

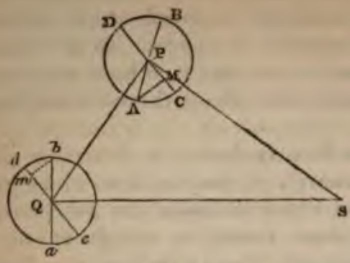
Now P E Q is the geocentric latitude which may be *observed*, and S E Q is equal to the sum of Q E ♈ and S E ♈, of which the latter is the *heliocentric* longitude, known in terms of the same quantities as before, and the former is the geocentric longitude, which may, therefore, be found by the subtraction of the heliocentric longitude from S E Q. Now this geocentric longitude may also be *observed*. Thus, then, the above process will furnish us with two results, which may be compared with observation, or two equations, involving the six unknown quantities, a, b, T, I, N, L.

Three such observations will, therefore, enable us to determine all those six quantities. One of them, however, N, may be determined by actual observation; for when the planet enters its node, the geocentric latitude and the heliocentric latitude both vanish, or it appears to go through its node at the time when it actually does go through it. The longitude of the point where the planet goes through the node, therefore, is equal to the longitude of the node, and may readily be observed. Thus, then, the conclusion drawn with regard to the position of the node may be verified, and will serve as one verification of the whole hypothesis. Moreover, the elements of one planet's orbit being calculated on the hypothesis of elliptical motion, of the existence of Kepler's laws, &c., these will enable us to determine certain elements of any other planet's orbit, simply by observing its periodic time; other geocentric positions may be calculated from these elements, and on the same hypothesis; and these being compared with observation, the verification of the original hypothesis may be carried to any extent. Now this is what has been done, and the planetary theory has thus been absolutely verified.

The Phases of the Planets.

Those planets whose orbits lie without that of the earth are called superior planets, and those whose orbits are within it, inferior planets. Being all of them opaque, spherical bodies, one hemisphere only of them can at any time be enlightened by the sun; viz., that hemisphere which is turned towards him. Moreover, since only one hemisphere of each can at any time be turned towards the earth, if they were even illuminated as to every part of the surface of each, but one hemisphere of any planet could at any time be visible.

Now, if the hemisphere on which we look, when we look at a planet, coincided always with its illuminated hemisphere, it is evident that the whole of that illuminated hemisphere would always be seen; but this is not the case. The hemisphere of the planet towards which we look when we look at it, does not always coincide with that hemisphere which is turned towards the sun, and enlightened by him; so that there is not any single planet of which we can see, except at particular times, the whole disc. In respect to the portion of the disc thus rendered, under certain circumstances, invisible, there is a great difference between the superior and inferior planets. The former, being considerably further from the sun than the earth is, have always a considerable portion, indeed the greater portion, of their enlightened sides turned towards us, whilst the latter sometimes present only their unenlightened hemispheres, and sometimes the whole of their enlightened hemispheres, to the earth, and their discs pass, in the intermediate period, through all those varieties of phase which characterize the changes of the moon. These appearances will readily be understood by the following diagram.



P and Q are two planets, of which P is the nearer to the sun, S. A C B and *a c b* are the hemispheres turned towards the sun, and, therefore, *enlightened.* A D B and *a d b* are the opposite hemispheres, which are in darkness. C A D is that hemisphere of the planet P which is turned towards the planet Q, or on which the observer at Q looks, the opposite hemisphere, C B D, being necessarily invisible to him by reason of the opacity of the body. Now, of this hemisphere only A C forms part of the *enlightened* hemisphere; the remainder, therefore, A D, is invisible to him. And if we draw A M perpendicular to C D, D M will represent the greatest width of that portion of the disc which is dark, and C M of that which is light. Now the value of D M depends upon that of the angle D P A, and this last is equal to the angle S P Q. Hence, therefore, it follows, that the thickness of the obscured part depends upon, and varies with, the angle S P Q; but the planet P is, in the course of a synodical revolution, brought into every possible angular position with respect to Q, S P Q goes, therefore, through every possible value between nothing and two right angles. And thus, the obscured part has every possible width, from nothing to the whole diameter of the circle; this is the case of an inferior planet.

Again, let us suppose Q to be looked at from P, the hemisphere on which the observer will look will then be *d b c*, and of this, *b* Q *c* will belong to the enlightened, and only *b* Q *d* to the dark side of the planet; so that *b* Q *c* will be the part of it actually seen, and drawing the perpendicular *b m*, *d m* will be the thickness of the obscured part of the disc, and this depends upon the value of the angle *b* Q *d*, being what is called its versed sine; also the angle *b* Q *d* is equal to P Q S. Now the greatest value of the angle P Q S is when the angle Q P S is a right-angle, or when Q P touches the orbit of P. Hence, therefore, the width of the obscured part has, in this case, a limit which it never exceeds; viz., that of the versed sine of the angle P Q S, when Q P S is a right angle.

For Mars, the greatest value of the angle P Q S is about 40°, and for Jupiter about 6°, and for Saturn about 5°; taking the versed sines of these angles, we find that the greatest width of the obscured part of the disc of Mars can never exceed one-tenth of the whole, that of Jupiter six thousandth parts, or that of Saturn three thousandth parts.

MERCURY.

MERCURY is an exceedingly small planet; his diameter is not more than two-fifths that of the earth, and his bulk but one-sixteenth, or three times that of the moon: moreover, he is at all times very distant from us, being, when seen under the most favourable circumstances, that is, when the most of his enlightened hemisphere is towards us, about as far off as the sun, and when nearest, about three-fifths the distance to him; so that he never appears under an angle of more than 12″, and his *apparent diameter* is sometimes as little as 5″. Being thus small, and thus remote, he would still be far more distinctly seen than he really is, were it not that he never occupies the dark portion of the heavens, his apparent distance from the sun never exceeding 28° 48′; so that we only see him in that portion of the heavens where the sun has just been setting, or where he is just about to rise: and thus the feeble light reflected by him is scarcely distinguishable by the naked eye from the brightness of the sky. Nevertheless, with a telescope he may readily be found, and, thus seen, he appears as he should, being an inferior planet, to have phases like the moon. After this planet has attained his greatest elongation of 28° 48′, every day finds him nearer to the sun, until he is lost, even to the telescope, in his beams. In a short time, however, he is discovered again on the opposite side of him, and keeps increasing, in that direction, his distance from him, until a greatest elongation is again, for the second time, attained, and he begins again to approach him, and thus appears continually to oscillate from one side to the other of the sun, as he moves forward in his path.

The duration of each such oscillation is from 106 to 130 days. There are certain periods when Mercury, being in conjunction with the sun, actually passes between him and the eye of the observer. These are called transits over the sun's disc. The centre of the earth being always in the plane of the ecliptic, as well as the centre of the sun, it is evident that this cannot occur unless Mercury also be at the time of its conjunction in that plane, or unless he be then at one of his nodes. Now, the nodes of the orbit of Mercury are situated in that part of the ecliptic which the sun passes through in the months of May and November. In order that a transit may take place, Mercury must therefore be in conjunction with the sun in those months. Such conjunctions return every third, fourth, sixth, seventh, tenth, thirteenth year, &c.

Mercury describes an orbit which is greatly more eccentric or elongated than the orbits of any of the other seven great planets, and which is inclined to the plane of the ecliptic at a much greater angle: moreover, he turns (as it is believed) upon an axis which is inclined to the plane of his orbit at a much greater angle than any of the other planets. So that, on the whole, he is remarkable among the planets for the great variations of his distance from the sun at different seasons of his year, for the obliquity of his path through space, which diverges widely from the planes of the orbits of all of them, and for the great changes of the temperature of his year, brought about by the great inclination of his axis to the plane of his orbit. The quantity of light and heat which any portion of his surface is at any given time receiving from the sun, is about seven times that which a similar portion of the surface of our earth receives when presented to him at the same angle; and thus the prevailing temperature of a summer on the surface of Mercury may be calculated to be greatly above that at which water boils here.

Mercury is *supposed* to have a dense atmosphere. The force of gravity on the surface of Mercury is somewhat more than three times that at the surface of the earth; so that to lift a mass of equal dimensions and density would require the exertion of three times as much muscular force as here. The mean density of Mercury is, however, only four-fifths that of the earth; so that we may, without any great stretch of imagination, conceive the various objects which present themselves to an inhabitant of that planet to be for the most part specifically lighter than the objects which answer to them here.

VENUS.

NEXT to the orbit of Mercury is that of Venus. Venus, although, like Mercury, she never appears but in that quarter of the heavens which the sun has just deserted, or where he is just about to appear, is, nevertheless, one of the brightest and the most beautiful of all the objects visible in the heavens, ranking, indeed, in splendour next to the moon herself. She recedes much further from the sun than Mercury, her greatest elongation being from 45° to 47°; by which quantity she sometimes precedes and sometimes follows him, being from three to four hours visible in the morning before him, on in the evening after him. The star which thus appeared at one time before the sun in the east, and at another after him in the west, the ancients imagined to be not one star, but two stars; they called it when a morning-star *Lucifer*, or Phosphorus, *Φωσφορος*, the star that brings with it the daylight*; and the evening-star they called *Hesperus*, (″Εσπερος).

The brightness of Venus, as seen from the earth, depends upon two causes; first, upon the shortness of her distance from the earth; and, secondly, upon the greater or less magnitude of that portion of her enlightened hemisphere which is turned towards it. These two causes conspire to render her brightness the greatest twice in each synodic revolution; when her elongation is about 40°. She may then be seen in broad daylight. The appearance of Venus through the telescope is exceedingly beautiful; when brightest, she presents at one time to the eye a small, but beautifully-defined and bright crescent; at another she is a half-moon in miniature, and then she becomes gibbous, until, when about to present the appearance of a full orb and a completed disc, she is lost in the sun's rays. Her disc is, for the most part, of unsullied whiteness: spots have, nevertheless, occasionally been seen; and, from what is believed to be a motion of these, she is asserted to turn round an axis inclined at an angle of 18° to the plane of her orbit, in a period of $23^{h}\,21'\,7''$. Moreover, it is asserted that certain appearances have been observed about the horns of this planet, indicating the existence of mountains of very great height, four times as high as the mountains on the earth's surface.

It is a very probable hypothesis, that the spots which are seen occasionally to float as it were in the dazzling brightness of this planet's disc, are in reality clouds, buoyed up in a dense atmosphere surrounding the planet, as our atmosphere does our earth; and that, in reality, the light by which we see this planet is not reflected by its solid mass, but by its atmosphere: so that, when we look at it, we see not the planet, but only the air that surrounds it, and in which float clouds, serving to break the intense glare of its sunshine. The apparent diameter of Venus varies, according to its position in reference to the earth, from $9''\cdot6$ to $61''\cdot02$.

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* Phosphore redde diem.—MARTIAL.

The diameter of Venus is nearly equal to that of our earth, and in volume she is only one-ninth less than it. Her mean distance from the sun is about five-sevenths that of our earth. She receives twice as much light and heat as our earth from him. The orbit of Venus is an ellipse, much more nearly approaching to a circle than the orbit of our earth, its eccentricity being only about one-tenth that of the earth.

The plane of this orbit is inclined at about 3° 23′ to that of the earth, and cuts it in a line, through which the sun passes in the months of June and December, the longitude of the nodes of Venus being 255° and 75°. Venus returns from any position in regard to the sun, to the same position again, or completes a synodic revolution in 584 days, and during the period of that revolution, she moves actually forward in her orbit through 316°, and thus in five synodic revolutions, which she completes in 5 × 584 days, or 2920 days, she has described 5 × 216°, or 1080° of longitude. Now, 2920 days are equal exactly to eight years of 365 days each, and 1080° are equal precisely to three times the complete circumference of a circle. Therefore, after eight years of 365 days, Venus always returns to the same place in her orbit, and to the same position in reference to the sun. Now, if Venus had an inferior conjunction with the sun when exactly in her node, she would manifestly interpose between the earth and sun, and to an observer at the earth's centre, or to one on the earth's surface, if situated in the line joining the centre of Venus and the sun, she would necessarily appear to pass over the sun's disc: moreover, this phenomenon may manifestly present itself to an observer situated elsewhere, and even when Venus is not precisely in her node. Its occurrence under these circumstances will manifestly be dependent upon the distance of Venus from the earth and sun; and it serves to determine that distance. Under each variation of these circumstances, the apparent path of Venus across the sun's disc is different; under the most favourable circumstances the transit might last 7^h 54′.

If Venus and the earth described circles, and they both moved uniformly in their orbits, with the mean velocities which we have supposed,—since they would be in the same relative positions after every period of eight years of 365 days, and since, in that same period, Venus would have returned to precisely the same place in her orbit,—it is evident, that a transit having at any time occurred, it would, after the space of eight such years, necessarily be followed by another under precisely the same circumstances, and so after the next eight years; and thus the transit of 1761 would be followed by another in 1769, a third in 1777, and so on; and one would have occurred in 1833. Now this is not the case: the transit of 1761 was indeed followed by one in 1769; but there will be no other until the 8th of December, 1874, and this will be followed by one on the 6th of December, 1882. This is easily explained:—neither the earth nor Venus move in reality with their mean or uniform motion; so that they do not in reality return into the same relative positions, after what we have supposed to be the period of a synodic revolution: nevertheless, the deviation is not so great as to bring them without the limits of a transit in one period of eight years, so that, in point of fact, we may always expect the return of a transit in eight years after it has once taken place.

The force of gravity on the earth's surface is only one-third of that on the surface of Venus; so that, if transported to that planet, we should almost be crushed by the weight of our own bodies, and three times the muscular force would be required to lift the same mass. The density of Venus is very nearly that of our earth.

Next in order is our Earth, of which we have already spoken, and of the circumstances of whose motion round the sun, and the form of its orbit, we know scarcely more than we know of the other planets.

MARS.

Beyond the orbit of the earth is that of Mars. The mean distance of this planet from the sun is about once and a half that of the earth, or, when nearest us, he is about half as far from us as we are from the sun. He receives about four-ninths the light and heat from the sun that we do; his diameter is about a half, and his volume is scarcely one-eighth that of the earth, or about six times that of the moon. His orbit is much more eccentric (nearly six times) than that of the earth, and he completes it in a period nearly double that of the earth; the length of his year is $686^{d}\ 23^{h}\ 30'\ 41''\cdot4$; his plane of revolution is inclined to that of the ecliptic at an angle of not more than $1°\ 51'$, and its synodic revolution occupies 780 days.

Varying in his distance from the earth, through a space equal to the whole diameter of the earth's orbit, which is more than two-thirds his greatest distance, and twice his least, he varies also very greatly in apparent magnitude. Moreover, presenting sometimes the whole of his enlightened hemisphere to the earth, and sometimes only part, he varies also greatly in apparent brightness. He is at once most bright, as far as this cause affects his brightness, and nearest to us, when he is in opposition to the sun, being then distant from us not more than half the distance of the sun, and appearing under an angle of about $18''$; his least apparent diameter is $4''$; his oppositions occur after two years and fifty days. In the month of August, 1719, this planet was in opposition, and at the same time in its perihelion, and its brightness was, under these favourable circumstances, so great, as to give rise to a superstitious terror.

The force of gravity on the surface of Mars is about one-tenth greater than that on the surface of the earth, but his density is greatly less,—almost in the ratio of one to ten. His day is nearly of the same length as ours, being $24^{h}\ 59'\ 21''$, and the axis about which he turns is inclined at an angle to the ecliptic, which is only 6° different from that of our earth; so that the distribution of temperature on his surface, and the variations of his seasons, are nearly the same as ours, except that each season is of twice the length, whilst the intensity of the sun's rays is not at any time more than four-ninths.

CERES, PALLAS, JUNO, VESTA.

Next to the orbit of Mars we find the orbits of four smaller planets—Ceres, Pallas, Juno, Vesta. The dimensions of the orbits of these are *nearly the* same, but their inclinations to the plane of the ecliptic are greatly different. Their periods are, Ceres 1681·3 days, Pallas 1686·5 days, Juno 1592·6 days, Vesta 1325·7 days.

The semi-diameters of their orbits are,

Ceres .	. 2·767245	radii of the earth.
Pallas .	. 2·772886	”
Juno .	. 2·669009	”
Vesta .	. 2·36787	”

The dimensions of all these planets are comparatively insignificant. So small are they, that they are undistinguishable to the naked eye, and their apparent diameters have never been measured with any tolerable accuracy, even with the aid of the most powerful telescopes. It is said, nevertheless, that the largest of them, Juno, cannot have a real diameter of more than 100 miles, or $\frac{1}{80}$th that of the earth. If this be the case, her surface is only the $\frac{1}{6400}$th that of the earth, and her bulk the $\frac{1}{512000}$th part.

JUPITER.

NEXT in order to the orbits of these four smaller planets, is that of Jupiter, the largest of all the planets. He is 1281 times greater in bulk than the earth, but his mean density is little more than one-fourth that of the earth; so that the quantity of matter actually contained in his bulk is not greater than that in the earth, in the same proportion in which his volume is greater: it is only about 331 times that of the earth.

The force of gravity on the surface of Jupiter is about eight times as great as that on the earth's surface; so that, standing on the surface of Jupiter, an inhabitant of our earth would have to bear up under a load eight times greater than the weight which he here sustains; and if he walked, he would have to carry eight times the present burden of his body,—eight times the muscular effort would be required to lift and to move any of the objects around him, that is here required for the same purpose; if he leaped, he could, with the same effort, leap only one-eighth the distance; and if he fell, he would fall with eight times the force. The distance of Jupiter from the sun is about $5\frac{1}{5}$th that of the earth; so that the sun's diameter there will appear only the $5\frac{1}{5}$th part of what it does here, and his area about $\frac{1}{27}$th, thus supplying to any given portion of the surface of Jupiter only $\frac{1}{27}$th the light and heat which the same portion of surface here receives. To compensate, however, for this deficiency of heat, every portion of the surface of Jupiter is, in each of his revolutions, brought under the influence of the sun again, after a much shorter interval than here elapses between one noon and another. Jupiter completes a revolution of his huge bulk upon an axis within himself once in every $9^{h}\ 55'\ 49''$; this rapid return of the sun must tend greatly to compensate the deficiency of heat on the surface of Jupiter arising from his distance. Again, although Jupiter receives, during his day, but a small portion of the direct light of the sun, yet he has four moons whose light must make his night as bright almost as his day. The axis about which Jupiter revolves is inclined at 86° 54′ to the ecliptic; so that his equator nearly coincides with the plane of the ecliptic. Very nearly in the same plane are the orbits of his four satellites; the first distant about six of his radii from him, the second nine, the third fifteen, and the fourth twenty-six. The first completes her revolution about him in $1\frac{7}{10}$th day, the second in $3\frac{5}{10}$th days, the third in $7\frac{2}{10}$th days, the fourth in $16\frac{6}{10}$th days. Thus, all appearing to revolve through his heavens nearly in the same path in periods varying between the extremes of from two to seventeen days, the *variety*, of their appearances, as seen by an observer on the surface of Jupiter, must be infinite. To an observer from the earth, there is no more interesting spectacle in the heavens. With a telescope of moderate power, they may be distinctly seen, at one time, all arranged in order, and in the same straight line on one side of the planet; at another, distributed partly on one side and partly on the other, but still in the same straight line. If watched attentively a few hours, they will be seen completely to change their relative positions, and, after a time, some or other of them will pass into the shadow which Jupiter throws behind him from the sun, and become invisible; or it will pass between Jupiter and the sun, and cast behind it a shadow on his surface, moving like a dark spot across his disc. By reason of the rapid revolutions of the satellites, these eclipses and transits return with great frequency. The first satellite is eclipsed every $42^h\ 20'$, the second every $85^h\ 18'$, the third every $7^d\ 4^h$, and the fourth every 17 days.

By reason of the small inclinations of the orbits of these satellites to the orbit of Jupiter, their eclipses, unlike those of the moon, return at every synodic revolution, except those of the fourth satellite, which is sometimes so far from Jupiter that, although its orbit is but slightly inclined, it yet lies above the shadow of the planet.

The periods of the eclipses of the satellites of Jupiter are given in the *Nautical Almanac*, and other astronomical calendars, because of their great utility in the determination of the longitude.

Each of these satellites, it is asserted, and with much probability, turns upon an axis within itself, precisely in the time in which it turns round Jupiter; as does the moon in the time in which she revolves round the earth; thus presenting always the same face to the planet. Jupiter's revolution round the sun is completed in $4332^d\ 14^h\ 2'$, or nearly 12 years: his equator nearly coinciding with the plane of his orbit, he can have very little variety of seasons. By reason of his more rapid revolution on his axis, and his greater diameter, it is manifest that the various points of his surface must be carried round much more rapidly in his daily motion than the corresponding points on ours. In point of fact, the velocity of the daily revolution of the equatorial regions of Jupiter is 26 times that of the earth, and the centrifugal force resulting from this greater velocity, 62 times greater. If, then, the flattening of the earth at its poles result from this cause, a much greater departure from the spherical shape may be expected to be apparent in the form of Jupiter: this, in point of fact, is found to be the case. The equatorial diameter of Jupiter is ascertained, by observation, to be greater by $\frac{1}{14}$th than his polar diameter—a result which coincides with theory.

SATURN.

NEXT in space to the orbit of Jupiter, is that of Saturn. His mean distance from the sun is about $9\frac{1}{4}$ times that of the earth; his diameter 79,042 miles, or about ten times greater than that of the earth: his year is 29 years, 5 months, 14 days of our time in length: he turns upon an axis inclined at an angle of 60° to his orbit, in $10^{h}\ 29'\ 16''$, and the diameter of the sun appears, as seen from his surface, only to subtend about $3'\cdot37$ in the heavens, covering a space of only about $\frac{1}{80}$th that which our sun appears to cover.

Thus Saturn derives from the sun only $\frac{1}{80}$th the light and heat that we do; the sun, however, returns more than twice as soon to the meridian of any place on his surface, and the deficiency of direct solar light and, perhaps, heat, is abundantly supplied by the reflected light of seven satellites; and a huge ring of attenuated matter, capable of reflecting the sunlight, girds the planet round his equator like a zone, but is separated from him by a space of at least 70,277 miles; the width of this ring is probably about 30,039 miles: it is, however, double, being composed of two concentric rings in the same plane, of which the larger is the most distant from the planet, and the width given above includes the space between the two. Whilst the ring is, in width, of these enormous dimensions, (about one-third of the diameter of the planet,) its thickness is so small as scarcely to be discernible. In the midst of this ring, the planet, as seen through a good telescope, appears to repose, presenting, at certain seasons, nearly the whole of the flat portion of it to the spectator; at another, having nothing but its edge turned towards him, and so that its existence is only recognised by the shadow which it casts in a dark line across the planet's disc. All these appearances are governed by the fact, that the plane of the ring, coinciding with that of the planet's equator, remains always parallel to itself. The ring of Saturn revolves with it, and the seven satellites of the planet have all their orbits in the plane of the ring.

URANUS.

THE most distant of all the planets from the sun is that discovered by our illustrious countryman, Herschel, in the year 1781, and named by him the Georgium Sidus, although it is more commonly called Herschel, or Uranus. Its distance from the sun is more than 19 times that of the earth, and 84 years are occupied in a single revolution through its orbit. The sun, as seen from it, has a semi-diameter of not more than 1′ 40″, and its surface appears 400 times less than it appears as seen by us, supplying $\frac{1}{400}$th part only of the light and heat. Its orbit is inclined only 45′ to the ecliptic, and there revolve about it six satellites in orbits nearly perpendicular to its own.

ON MUSICAL GLASSES.

If we were to enumerate all the instances in which a fear of too great an expense puts an unfortunate check to any attempt to avail ourselves of rational pleasures otherwise desirable, we should find that a great number of them are occasioned by a very erroneous impression of the difficulties to be overcome. By the exercise of a little reflection, and a little ingenuity, we might greatly enlarge the scope of our pleasures,—pleasures every way removed from all that is gross and debasing,—with an outlay which comes within the power of large classes of society, who, at present, consider themselves to be shut out from their enjoyment.

Among such sources of pleasurable enjoyment, are musical glasses, an assemblage of which forms an instrument which appeals to the feelings in a manner which few other musical instruments can equal: the liquid sweetness, the roundness of volume, the swelling crescendo and diminuendo effects, and the thrilling intensity of the tones elicited from these glasses, are such as no one with a spark of musical feeling can be indifferent to; and yet, it is almost excluded from the social circle, except in the more wealthy classes: those even moving in the middle ranks of life are deterred from incurring the expense of such an instrument; but it will be seen that this is not necessarily so. A tolerably correct musical ear, and a little ingenuity, will be sufficient, at a few shillings expense, to bring this pleasure within the reach of most domestic circles, and to add one to the many sources of those enjoyments which are appreciated better in this than in any other country, by the name of "fireside amusements;" and those well-wishers to mankind who strive to elevate the tone of moral feeling, and to bring out all the better and most ennobling traits of the mind, well know how to value any endeavour which has the effect of increasing the charms of domestic society, and to render more endearing and attractive those associations which so irresistibly steal over us when connected with the word "home."

We will give a short history of the origin and present construction of this instrument, and then detail the method by which it can be obtained in an economical and convenient form.

The first mention which we have of glass vessels being employed as musical instruments, is found in a letter written by Dr. Franklin to Father Beccaria. The Doctor calls them *armonica*, from the Greek word for harmony. We will give a short portion of the letter in Franklin's own words, as they are very characteristic of the subject.

He says, "You have doubtless heard the sweet tone which is drawn from a drinking glass, by pressing a wet finger round its brim. One Mr. Puckeridge, a gentleman from Ireland, was the first who thought of playing tunes formed of these tones: he collected a number of glasses of different sizes,—fixed them near each other on a table,—and tuned them by putting into them water, more or less, as each note required; the tones were brought out by pressing his fingers round their brims. He was unfortunately burnt here, with his instrument, in a fire which consumed the house he lived in. Mr. E. Delaval, a most ingenious member of our Royal Society, made one in imitation of it, with a better choice and form of glasses, which was the first I saw or heard. Being charmed with the sweetness of its tones, and the music he produced from it, I wished to see the glasses disposed in a more convenient form, and brought together in a narrower compass, so as to admit of a greater number of tones, and all within reach of hand to a person sitting before the instrument; which I accomplished, after various intermediate trials and less commodious forms both of glasses and construction, in the following manner." Franklin then proceeds to describe his instrument, the construction of which appears to be this:—He had twenty-four glasses blown to a hemispherical shape, and varying from nine inches to three inches in diameter; the tones of these glasses were tested by means of a harpsichord, and when any inaccuracy of tone was observed, the exterior surface was ground down to a greater thinness, whereby the tone was lowered; in order to prevent any of them being *lower* in pitch than was required, he had five or six made nearly of one size, and then ground them down to the proper tones.

The intervals between the tones of the glasses were so arranged that he had three complete octaves. When the requisite accuracy was attained, he arranged the glasses in the following manner:—A box or case was made, three feet long and eleven inches in width, and the same in depth at the larger end, while the width and depth diminished to five inches at the other end. Through the middle of this box, from end to end, proceeded an iron rod, an inch and a half thick at one end, and half an inch at the other, tapering from end to end in the same way as the box which contained it. The bottom of each glass was perforated, and the perforation surrounded by a collar or neck, the diameter of which was one inch and a half in the largest glass, and half an inch in the smallest; into each neck a cork was tightly fitted, through which a hole was bored. The glasses, thus prepared, were then fixed on the iron rod in succession, one in the other's hollow, with the plane of the periphery in a vertical direction. If we suppose twenty-four tea-saucers or basins placed one in the other's hollow, and then turned up on their edges, and a rod passing through them all, it will give some idea of Franklin's arrangement. The glasses were not allowed to touch each other, as the corks were slightly projected to prevent such contact.

The iron rod was not *fixed*, but moved in a gudgeon at one end of the box, while at the other it was connected with a wheel round which a strap passed, which communicated with a lever which was moved by the foot: on pressing this lever, then, the rod acquired a rotatory motion, and the glasses, being firmly fixed to the rod, were carried round with it; the exterior surfaces of the glasses being then slightly wetted, and the finger entirely free from grease, each glass, as its particular tone was required, was touched by the finger in the course of the revolution of the rod, and thus the tones were elicited. Franklin says, "To distinguish the glasses more readily to the eye, I have painted the apparent parts of the glasses within-side, every semitone *white*, and the other notes of the octave with the seven prismatic colours; viz., C red, D orange, E yellow, F green, G blue, A indigo, B purple, and C red again, so that the glasses of the same colour (the white excepted) are always octaves to each other."

Franklin describes the tones as produced from this instrument as being superior in mellifluous sweetness to anything he ever heard before; but it is obviously an arrangement which is exceedingly liable to disturbance; any want of horizontal precision in fixing the glasses on the rod, and any slight loosening of the contact between the glass, the cork, and the rod, would throw the whole apparatus out of repair, while the expense of it must be enormous; as the Doctor advises that *six* glasses of each size should be formed, in order that the one which approaches most nearly to the required pitch might be selected; thus the number made would be nearly 150.

Dr. Edmund Cullen, of Dublin, subsequently proposed a modification of this arrangement, apparently not so much in the mode of fixing the glasses, as in the choice of tones and harmonic chords which would be productive of the most pleasing effects; but the objection as to difficulty of construction, and costliness, applies to this as well as to the former construction.

It has been proposed to arrange the glasses on a wheel which turns rapidly in a horizontal direction, but supposing that to succeed, as far as mechanical arrangement is concerned, every plan of this kind is subject to the serious objection that wheel-work inevitably creates a *noise*, the effect of which is a sad interference with the melodious strains of the glasses.

Subsequently, however, all wheel-work, and similar machinery, was discarded, and the glasses were fixed in double rows upon a horizontal board, and the tones produced by the motion of the wetted finger upon the rim of the glasses, instead of the stationary finger upon moving glasses.

A few years ago, Mr. Tait devised a method of preparing the harmonica, which presented a considerable improvement on previous methods. His glasses are shaped somewhat similarly to a glass sugar-basin, and having been blown to a size which experience has determined to be nearly fitted for the production of the requisite tones, they are brought to the proper pitch by coating a portion of the surface with a resinous composition, which has the effect of retarding the velocity of the vibrations, and, as a necessary result, lowering the tone. These glasses, thus tuned to the proper pitch, are arranged in a double row in a case fitted for the purpose; the upper edges of all the glasses are on the same level, and the tone is produced by passing the moist finger round the edge of the glass whose tone is required. This harmonica is by far the most perfect which has ever been constructed; no friction or grating noise of wheel-work interferes with the melody of tone, and the whole arrangement, whether considered as a source of beautiful musical effects, or merely in its exterior fittings, is a very elegant and delightful ornament to the drawing-room.

But the latter expression at once brings us to the object of this article. The harmonica which we have just described, beautiful and perfect as it is, is necessarily attended with an expense so great, that it *is* utterly unavailable for the *popular* extension which we wish to see attached to this beautiful instrument.

An economical mode of attaining the object, with moderate accuracy, has been frequently adopted, thus: the glasses are chosen as nearly as can be of the proper pitch, and the requisite adjustment is made by means of *water*, which, being poured into the glass, lowers the tone: but this is, for many reasons, an imperfect mode; the freedom and extent of the vibrations of the glass being retarded by the weight of the water contained in it; besides which, evaporation is constantly going on from the surface of the water, and the relation between the tones of the glasses containing water and of those which do not, is constantly changing, and requires very frequent adjustment.

Now, the mode in which we propose these glasses to be chosen, is one first pointed out and practised by Mr. Charles Tomlinson, and which we know from experience to be available for the purpose. It is to place a great number of soda-water glasses, goblets, and wine glasses, in a convenient way for ascertaining their tones (and this might be done at any warehouse where such things are sold), and choose the lowest tone which could be selected from among the soda-water glasses; ascertain its value, and make it the fundamental, or key note, to which all the others might be referred. It matters not much what that note is, provided the others are chosen with reference to it. In three different instances, the writer of this article was enabled, in an hour and a half or two hours, to select very accurate and beautiful sets, embracing a range of 1½ or 2 octaves; about two or three soda-water glasses, eight or ten goblets of different sizes, and three or four wine glasses, will, in general, produce two octaves complete, with an additional semitone, in case it might be desired to play in another key. The most useful semitones which could be chosen would be the minor seventh and the minor fourteenth, above the fundamental note of the series, or those two notes which are respectively half a note below the octave and the double octave to the key note; these two additional semitones will enable the performer to play in a key a major fourth above the fundamental key of the set.

The glasses thus chosen (and a moderately correct ear will readily ascertain the relation which the tone of a flute bears to any glass whose tone is being elicited), they may be placed in a box, in any order which may be thought most desirable for facility in playing; and as they are of unequal heights, the smaller glasses may be elevated on small blocks of wood, so as to bring them all to an equal height; the foot of the glass may then be secured to the case by means of two or three wooden screws, similar to those used in a swing dressing-glass, the screw being within a quarter of an inch of the foot, the head or nob of the screw will firmly grasp it, and retain it in its place.

Various different modes of arranging them have been proposed, but that of Dr. Arnott is, perhaps, the most convenient. He places them in a zig-zag double line, so that, in ascending the series from the lowest to highest, the finger would describe a course analogous to the tacking of a ship in a contrary wind. The advantage of this arrangement is, that all the notes which, in printed music, are on the *lines*, are, in one represented by glasses, in one line, while those which occupy the spaces are placed in the other line: thus, suppose our fundamental note is the lowest C on the flute; one line of glasses would contain C, E, G, B, D, F, A, C; while the other line would contain (ascending in the same way) D, F, A, C, E, G, B. There is another advantage attending this arrangement; as it is very easy to play with both hands on this instrument, it is desirable that the harmonic thirds, which so continually occur when a duet is played, should be placed in our arrangement as prominently and conspicuously near each other as possible. In Arnott's system they are immediately next each other in the same line, and, therefore, mistake is impossible.

Should the choice of the glasses devolve upon one whose musical ear, as it is called, is not always sufficient to assure him when he has attained unison between the flute and any glass, the following plan has been found very available: suppose, for the sake of making our meaning more plain, the lowest note procured is C; if the note D be then played strongly on the flute, the glass on the table which happens to be in unison with it will sound sympathetically with it, without being itself struck or rubbed by the finger[*]. This is one of the most beautiful facts in the science of sound: if two bodies capable of producing sonorous vibrations have such an elastic tension that they will vibrate in equal times, the sound elicited from either one of them will, when they are placed near each other, excite the vibrations of each other; and this is, perhaps, the most delicate test which we could possibly have of the unison of two tones.

When the D is thus produced, E might be sounded in a similar way, and if the sympathetic note E were heard from any one glass, that may at once be selected. The same remarks apply, in precisely the same manner, to all the other notes, and we thus make the sounds themselves assist us in determining that which our own perception is not sufficient to accomplish.

Every one at all conversant with music knows that a very large range of tunes can be played within one octave, and a selection of even eight only, such as we have described, will be found a prolific source of enjoyment; and it will be productive of pleasurable feelings to the minds of all who are alive to the poetry of music, to find that their taste can be gratified from instruments which were made for a very different purpose, and at so very small an expense.

Many suggestions have been made as to the best mode of exciting the vibrations of the glass; some have used cork instead of the finger; others have employed leather; while others, again, have dipped the finger into powdered chalk; but it is doubtful whether anything is so available and consistent with the object in view as the moistened finger: the water may be slightly impregnated with alum, or with lemon-juice, or a few drops of muriatic acid, or of pyroligneous acid, may be put into the water; but, with a little tact and a little practice, pure water will do perfectly well.

It may also be remarked that the tone is best elicited when the finger is moved *from*, and not *towards* the player: this remark was made by Franklin with reference to his revolving apparatus, and it applies equally to the other systems. The glasses should likewise be frequently sponged, to remove any dust or grease from the edges; and previously to performance the hands should be washed in warm water, for the purpose of softening the skin of the finger; they must then be well dried, and then dipped in *cold* water, to produce the tone. A glass of cold water should be contained within the case, as near to the performer as possible. When set aside, a wooden cover should be provided, to protect the glasses from dust and injury.

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[*] One of the harmonic notes, a third or a fifth, for example, will cause the fundamental note to vibrate, and *vice versâ*; but this sympathetic vibration is of a very faint character, and will never be mistaken for the full and decided sympathetic tones produced by the vibration of one body in unison with another sounding body.

Before we conclude, we will shortly allude to the mode in which a glass is divided during the production of the tone elicited from it: the full consideration of the subject involves scientific principles of a character unsuited to our present purpose, which is purely *practical* and *practicable.* We intend, in a future paper, to enter pretty fully into this subject, but at present we will merely submit the following remarks. On the first impulse being given to the glass, it assumes an elliptical or oval shape, which the great elasticity of the substance prevents it from retaining; it therefore recedes to its former circular position, and, in so doing, acquires such a velocity that it is carried as far *within* the proper boundary as it was originally *without* it: the recession from this position again, carries it a second time beyond the boundary or circle of repose, and thus it reciprocates backward and forward: it necessarily results from this mode of action, that, at the second instant, it forms an ellipse at *right angles* to the ellipse formed in the first case. Now the change from one ellipse to another is so rapid that they alternate in some cases many hundred times in a second, and at every change impart a new impulse to the surrounding air, which impulse is conveyed by it to the ear; the pitch or key of the note which we hear depends rigorously and entirely on the number of those changes in a given time, and as the glass is small, so does the rapidity of these changes increase, and thus the higher tone from a small glass is accounted for.

Such is the rapidity with which these changes from one ellipse to another succeed each other, that, were it possible for us to detect the motion of the glass, we should see them both at once, for the small ends of both ellipses would appear to be co-existent at the same instant, separated by a depression or *node*, where the two ellipses cut each other. But though we cannot see these motions in the glass, we can detect their existence by observing the agitations produced in any water contained in the vessel; the surface appears broken up into *four* systems of undulations, proceeding from the circumference towards the centre; each of these systems proceeds from the small end of one of the ellipses. Now if we vibrate the glass by drawing a bow across its edge, these four systems, or fans, are stationary in position; but when we pass the finger round the edge, the motive power is continually shifting, and the double ellipses travel round with it, and it will then be found that the liquid surface is broken into four equidistant fans of undulæ, which travel round with the same velocity, and in the same direction, as the finger: it is in this way curious to remark that when the bow is applied, a *small end of one of the* ellipses invariably forms beneath it; but when the finger is used, it is always at a node—the point of division between two ellipses*.

This, then, is the simple principle on which these beautiful sounds are produced, and is one of the innumerable claims by which our attention is drawn to the extremely interesting science of acoustics.

We may here observe, that the perfect character of the tones producible from glasses subjects them to a defect which less perfect instruments would render inappreciable; we mean, the interference of two tones, which, in musical language, is said to produce discord. Suppose the note G, for instance, is occasioned by 100 vibrations in a second, and the note A by 108 vibrations; at each of the 100 vibrations the air between the glass and the ear is thrown into a sort of *wave* of sound, which wave recedes, and is again propelled forward, 100 times in a second; the same air is agitated by the other glass 108 times in a second; therefore, at the beginning of the double vibration, the forward advance of the waves is nearly equal in both cases, but at the end of six seconds, the quick vibrations would have so far gained upon the slower, that the impulse *forward* imparted to the air by one glass, is nearly as strong as the recessive impulse from the other, and the air does not *move at all;* it would be found, therefore, as long as those two notes are sounded together, there would be an interval of *silence* every six seconds. This repetition of stoppages, or *beats*, as they are called, is the sole cause of the discord which we hear when two adjoining notes in the scale are heard together. In any instrument in which the sound ceases soon after the exciting cause is removed, this inconvenience is not felt; but when the sound, as in musical glasses, continues many seconds after the finger is removed, several successive notes in the same time are lingering in the ear at the same moment. If they happen to be thirds, or fifths, or octaves, the effect is heightened, but if a second, or a seventh, the beat or jarring commences.

There is an analogy to this interference afforded in a very familiar phenomenon: when a stone is thrown into a lake or pond, a series of concentric circles of waves or undulations emanate from it: these circles consist of alternate elevations and depressions of the liquid surface. If now another stone be thrown in at a short distance from the first, a similar series will emanate from that also, and the two series will soon meet each other. If it happen that an elevated ridge from one system coincide with an elevated ridge from the other, the result will be an increased elevation; but if a depression from the one meet an elevation from the other, the surface, by the balance of forces, remains at its original level, and no wave exists there. So it is with sound. If two progressive waves coincide, the effect is an increased tone, but if a progressive meet a recessive wave, both are neutralized.

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* We have here described the action of the fundamental tone of the glass, but it must be remarked that each glass is capable of affording as many as five or six distinct notes, varying in an ascending order, and occupying a place in several octaves; yet it happens fortunately that the wet finger produces invariably the lowest tone, or that tone which prevails when the glass is struck, and it is only by long practice and great tact that the upper tones can be produced by the finger: these, however, are only necessary for the purposes of scientific inquiry, and need not be attempted by the reader who wishes to employ these glasses for their musical effects alone.

This defect is, however, so small, when brought into comparison with the excellences of the instrument, that we must not allow it to deprive us of the pleasure derivable from those excellences. In fact, those excellences themselves are the cause of its defects.

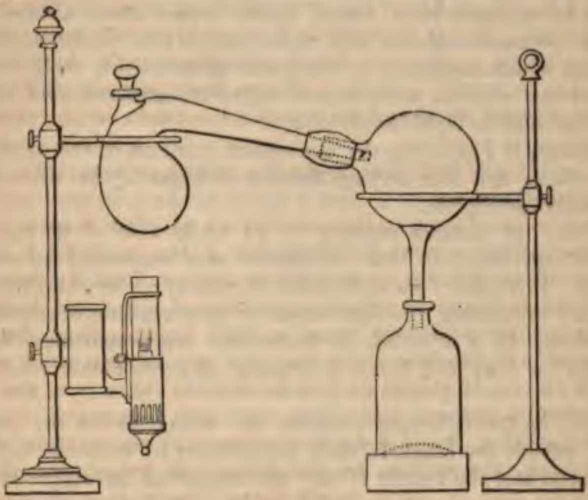
Should this short article have the effect which is desired, viz., to extend the range of musical sources available for the large bulk of the community, it will add one to the many instances in which science, if sought for and properly appreciated, will conduce quite as much to the *pleasures* as to the solid *advantages* of society. T.

A POPULAR COURSE OF CHEMISTRY.

XI.

NITROGEN.

FOR the production of *nitric acid*, which is the *fifth* compound of *nitrogen* and *oxygen*, you require a little apparatus, to be arranged as here shown.



This consists of a tubulated glass retort (capable of holding about a pint), whose neck is fitted to the mouth of a quart glass receiver, "quilled," as the glass-blowers call it; that is, having a small pipe or tube, about the size of a quill, attached to its lower part, so that any fluid products of distillation may be conveyed through it into a bottle; the bottle here shown may hold about six ounces; the quill of the receiver is to be placed in its neck loosely, and without any cork; the retort and receiver are to be properly adjusted on "retort stands," as in the sketch.

Put into the retort four ounces of crystals of nitre, and then, by means of a small, long-necked glass funnel, pour on them an equal quantity of strong *sulphuric acid;* remove the funnel into a basin of water, to wash off the acid, insert the stopper of the tubulure, and then proceed to apply heat very cautiously to the bottom of the retort; this is best done by means of an Argand's lamp (as shown in the sketch); this can be purchased at any philosophical instrument maker's.

Increase the heat gradually, and you will find fumes evolving from the materials; these fumes, passing along the neck of the retort into the receiver, become cooled, and condensed into drops of a colourless liquid, which runs down the quill into the bottle; this liquid is *nitric acid.*

I do not advise you to carry on the distillation too far; that is to say, do not continue the heat until the contents of the retort become dry, for very likely it may then break, and there is no occasion to run the chance of such an accident; but when you have collected about half a fluid ounce of the acid in the bottle, you may stop the operation by taking away the lamp, and, when the apparatus is cool enough to handle, by lifting up the receiver you can remove the bottle, and insert its stopper so as to preserve its contents.

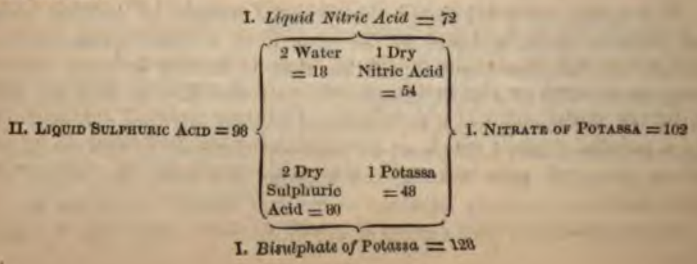
The theory of the production of *nitric acid* in this process is as follows:—

Nitre is a *nitrate of potassa*, that is, a compound of *nitric acid* and the alkaline *oxide* of *potassium*, or *potassa;* when *nitrate of potassa* is acted on by *sulphuric acid*, which, in the *liquid* state, always contains water, the *potassa* combines with the *sulphuric acid*, producing *bisulphate of potassa*, which remains as a white salt in the retort, whilst the *nitric acid* combines with the *water* thrown off by the *sulphuric acid*, and distils over as *hydrated* or *liquid nitric acid.*

Perhaps, if I give you the equivalent numbers of the substances in the first place, and then arrange them in a diagram, the matter will be more readily intelligible.

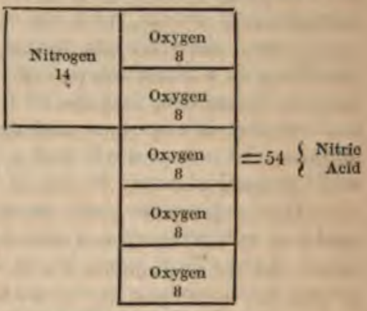
Nitrate of potassa consists of 54 *nitric acid* + 48 *potassa;* its equivalent, therefore, = 102. *Sulphuric acid* consists of 16 *sulphur* + 24 *oxygen* + 9 *water;* its equivalent, therefore, = 49; but, for the process we are examining, *two* equivalents or proportionals of *sulphuric acid* are required, 49 × 2 = 98, to decompose *one* proportional of *nitrate of potassa* = 102; and hence the reason why you obtain *bisulphate of potassa.*

Now, in the following diagram the acting bodies are printed in capitals, outside the brackets, their components in common type, inside the brackets, and the results of the decomposition in italics at the top and bottom.


I. *Liquid Nitric Acid* = 72
2 Water = 18
1 Dry Nitric Acid = 54
II. LIQUID SULPHURIC ACID = 98
I. NITRATE OF POTASSA = 102
2 Dry Sulphuric Acid = 80
1 Potassa = 48
I. *Bisulphate of Potassa* = 128

I think from this arrangement it will now be clear to you, that when 2 proportionals of *liquid sulphuric acid* act on 1 proportional of *nitrate of potassa*, that the acid throws off its 2 proportionals of water, becomes *anhydrous*, that is, and in such state combines with the 1 of *potassa*, producing 1 proportional of *bisulphate of potassa;* whilst the *nitric acid*, at the same time quitting the potassa, and meeting with the 2 proportionals of *water*, distils over in company with it, producing 1 of *liquid nitric acid*, consisting, that is, of 1 of *real nitric acid* = 54 + 2 water = 18 = 72.

It may now, perhaps, be as well to inform you at once regarding the *composition* of nitric acid, because all the experiments about to be mentioned depend upon its very large proportion of oxygen. It consists of 1 proportional of *nitrogen* = 14, and 5 proportionals of *oxygen* = 40, as shown in the accompanying symbol.



Its equivalent number is, therefore, = 54, and in such proportion it unites with bases to form *nitrates;* for example, 54 nitric acid + 48 potassa = 102 *nitrate of potassa:* but we cannot obtain the acid in its insulated state, that is, *anhydrous*, as it exists in nitrates; the very strongest nitric acid known to the chemist, which has a specific gravity of 1·5, always contains 2 proportionals of *water* = 18, so that you must regard liquid nitric acid as a compound of *real acid* = 54 + *water* = 18, giving you the equivalent 72.

Now the small quantity of nitric acid that you have obtained in the above process, (whose theory I trust I have rendered intelligible,) is by no means sufficient for all the experiments about to be mentioned; but, as nitric acid is a very abundant article of commerce, you had better purchase about a pound of it at the "operative chemist's," and keep it in a glass-stoppered bottle. *Pure* nitric acid is *colourless*, or having only a very slight tinge of straw colour; such acid you must obtain. There is, however, a variety of it which is of a dark orange colour; this is generally obtained from very impure nitre, and the colour is owing to *nitrous acid gas*; such acid is most improperly called *nitrous* or *nitros acid;* and another name for it is *aqua fortis*. With this impure acid we have nothing to do, and therefore I shall exclusively direct your attention to the properties of pure nitric acid.

It is a very corrosive substance, intensely acid, and exerts a powerful action upon most organic and inorganic bodies; therefore you must be cautious how you handle the bottles or glasses containing it, for if any of it happens to drop on the hands, it instantly destroys or kills the part, staining the cuticle of a bright yellow. In larger quantity this acid produces a painful wound, which is very difficult indeed to heal; if by any accident you get splashed with the acid, wash it off with water immediately.

As an example of the powerful action of nitric acid upon organic bodies, place a few strips of white glove leather, or parchment cuttings, in a saucer, and sprinkle them over with a few drops of the acid; they will instantly begin to crinkle and shrivel up, exactly as though scorched with a hot iron; their texture is entirely destroyed.

Take a clean quill, dip it into nitric acid for half a minute, then take it out, and wash it well in water; you will find that you have thus permanently stained the quill of a bright yellow colour, and I believe that the yellow quills sold at the stationer's are mostly stained by this simple operation.

Treat a slip of "lance-wood" in a similar manner, and you will find its pale buff colour changed to a bright yellow, orange, or brown, according to the time that you leave the acid on it. Wood thus stained is called "lemon-wood," and is much used for archery bows, whip stocks, and walking sticks, as it looks very well when French-polished or varnished.

Take a piece of white flannel, or kerseymere, and dye it blue by immersion in a dilute solution of indigo in sulphuric acid*; squeeze out the superfluous dye, then spread the dyed cloth on an earthenware plate, and sprinkle it over here and there with nitric acid; leave it for a few minutes, and then you will find a yellow colour produced wherever the acid has touched.

This experiment will serve to explain how yellow patterns are produced upon blue or green cloth table-covers; these articles are often very rotten in the yellow parts, because the acid has either been applied too strong, or left too long on the cloth, so that, in addition to destroying the dye, or changing it to yellow, it has destroyed the texture of the cloth; but, when carefully applied by skilful workmen, no injury results to the woven fabric.

Should you happen to wear a blue or a green coat during these experiments, and accidentally or intentionally splash it with nitric acid, you will find yellow spots appear on the cloth wherever the acid has touched; the cloth will very soon fall into holes in those parts. When chemists find their clothes thus damaged by any acid, they are in the habit of applying a solution of ammonia (*liquor ammoniæ*), to *neutralize* it, and thus to prevent the texture of the cloth from being destroyed; in some cases, even the original colour of the dye returns after this neutralization, but never if the acid has been very concentrated, or long applied.

Nitric acid exerts a powerful action on some combustible bodies, as may be shown in several instances, but I have selected only one or two.

Take five grains of *chlorate of potassa*, powder it very fine in a clean earthenware mortar; then shake out the powder on a sheet of paper; wash the pestle and mortar from all traces of the salt, wipe it dry, and then reduce to powder in it four grains of *sulphuret of antimony*, commonly sold as "black antimony;" place this powder on the paper by the side of that of the chlorate of potassa; now, with a feather or piece of stiff paper, mix the two very intimately together. You must not use a knife or a bit of wood for this purpose, because the friction of such hard substances might cause the mixed powders to explode; above all, do not attempt to make the mixture in a mortar, for then you would have a dangerous explosion; but proceed with a feather or a bit of paper, as directed, and then you are safe. Having made the mixture, take a few grains of it gently on the end of the blade of a dessert knife, or piece of card, and drop it suddenly from off the knife or card into a tall ale glass, containing five or six drops of nitric acid. There instantly results a flash of light, and a beautiful column of dense white smoke rises swiftly out of the glass. There is no explosion, at least no explosion in the common acceptation of the term; and there is no danger whatever in making the experiment.

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* Vol. II., page 293.

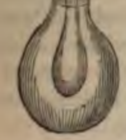
If you have any of the mixture remaining after having made this experiment as often as you like, it is best preserved in a card pill-box. Never put it into a stoppered phial, because very likely the friction of the stopper may cause it to explode.

It is as well to have some place set apart in the laboratory for keeping these explosive compounds in safety, and I think that a sheet-iron box is, perhaps, as good a "safe" as any; of course I mean that it should have a secure cover. You may keep all the dangerous substances in it, and supposing that they do take fire, or explode, there would be little or no chance of injury resulting to the laboratory. If you leave your laboratory for a week or two, you should be very careful about these matters; for, supposing that the mixture we are speaking about was accidentally left near bottles of sulphuric or nitric acid, and either of these bottles broke, coming into contact with the mixture, flame would result, and your laboratory might be destroyed by fire. I would advise you, in the first place, never to keep large quantities of any inflammable or explosive materials, and, in the next, if you leave your laboratory for any time, to place all such things in safety. You might place the iron box containing them in the grate, or on the hobs of the grate, and then, supposing they spontaneously exploded with the evolution of flame, very little danger would ensue.

The next experiment that I am about to describe requires a good deal of caution, for it is rather dangerous in unskilful hands, but yet so exceedingly illustrative of the singular power of nitric acid on inflammable bodies, that I am induced to mention it to you. Take a wide-mouthed phial, "a half ounce wide mouth," as it is commonly called, and tie it firmly across the end of a stick about four feet long; then pour half a drachm of *oil of turpentine* into an earthenware plate, which, by the way, you had better set out of doors; put into the phial half a drachm of *sulphuric acid*, and a drachm of *nitric acid;* then hold the stick at arm's length, and suddenly pour the mixed acids upon the turpentine, which suddenly bursts into flame, and a column of dense black smoke ascends. Sometimes this experiment will not succeed, but it is sure to do so if the plate be made pretty warm before the turpentine is poured into it. Remember that this experiment requires caution, and it should never be attempted in a small room, because the flame is very great, and the smoke very annoying.

When this property of the inflammation of turpentine by the mixed acids was first discovered, some folks proposed it as a destructive agent in warfare, as follows:—

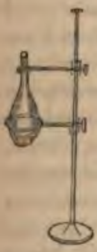
A glass bulb, about six inches in diameter, to be filled with oil of turpentine, and a smaller bulb or flask, containing the mixture of nitric and sulphuric acids, immersed in this, the necks of both being properly secured; such an arrangement being thrown against an object, both glass bulbs would smash, and the turpentine inflame upon meeting with the acids, producing a flame very difficult of extinction; and thus it was imagined that the "Greek fire" would be equalled, if not surpassed, in destructive power.



The action of nitric acid on alcohol is very energetic and very remarkable. You can safely make the experiment, if you follow the directions I am about to give you.

Take a clean, dry Florence flask, and set it on some sand, either out of doors or beneath the chimney; then pour into the flask about half an ounce of strong spirits of wine, (*alcohol,*) and a quarter of an ounce of *nitric acid;* in the course of a few minutes, a cracking noise will be produced in the flask, succeeded by a copious rush of fumes from its neck, which fumes are those of the *nitrous æther*, highly inflammable, and if you hold a slip of lighted paper in the stream of vapour, a large blaze of pale flame will result: it is a very good experiment in illustration of the change of state which bodies suffer when made to act chemically upon each other; for the nitric acid and the alcohol, when *mixed*, were *liquids*, but become converted into *vapour*, on account of their mutual affinity.

Sometimes the action is a considerable time before it commences; and you may hasten it, by slightly warming the bottom of the flask by the flame of a spirit-lamp, or you may place the flask on some warm sand; in either case, if the action once commences, it will go on without any further application of heat. I do not advise you to employ a much larger quantity of materials than that already directed, and be sure to bed the flask firmly in the sand, so that there may be no chance of the jerks or concussions turning the neck of the flask in the direction of your face, because, perhaps, you may suffer injury if the mixture spirts out of the vessel. You may arrange the flask on a retort-stand, as here shown, and then there will be no chance of any accident happening, because the upper ring prevents it from taking an oblique direction.



Let us now examine the action of nitric acid upon some of the metals; and copper will suit the purpose very well. Put a few bits of clean copper wire into a small wine-glass, with some nitric acid diluted with two parts of water; a violent action commences, the copper dissolves just as already shown at page 178 for the production of nitric oxide, and the blue solution is called a *nitrate of copper.*

If you were asked the question, "What *is* nitrate of copper?" your answer would very probably be, "It is a compound of *nitric acid and copper.*" Such, however, is not the case; for there *is no such thing* as the *nitrate*, *sulphate*, &c., of a metal; acids will *not unite directly* with metals; they require to be oxidized first, and *then* the *oxide* and *acid* unite; so that, as in the above instance, the resulting *salt* is not a *nitrate* of *copper*, but a *nitrate of the oxide of copper*.

When you put *nitric acid* and *copper* into contact, a portion of the acid is *decomposed;* part of the *oxygen* goes to the *copper*, forming *oxide of copper*, which is instantly dissolved by an *undecomposed* portion of the acid, forming a solution of *nitrate of oxide of copper;* so that you see the metal is oxidized at the expense of part of the *oxygen* of the acid, and that a portion of the acid thus undergoes decomposition to furnish the oxygen, must be evident to you by the evolution of *nitric oxide gas* that takes place during the process; it was thus that we obtained the gas on a former occasion, by robbing nitric acid of part of its oxygen by means of copper. If the nitric acid combined *directly* with the copper, there could be no production of *nitric oxide.*

In order to prove to you that *oxide of copper*, and not *metallic copper*, exists in the nitrate, take some of the blue solution that you have just obtained, or that resulting from the process at page 178; pour about an ounce of it into a small saucer, and evaporate it over a lamp; you thus expel the water, but continue the heat, the nitrate begins to alter its colour, fumes of nitric acid escape, and at length, when these cease, there remains in the saucer a purplish *black* powder—namely, *oxide of copper:* let this cool, and then pour on it a little nitric acid; it dissolves immediately, and furnishes you the blue solution; you have no evolution of fumes, because the acid is not required to furnish oxygen; the copper has already got its just proportion, and therefore unites directly with the acid, producing *nitrate* of *oxide* of *copper.*

The action of nitric acid on *mercury* will also afford you another instructive instance of the matter in question. Put a globule of the metal, about the size of a hazel-nut, into a wine-glass, and pour upon it, by degrees, about half an ounce of nitric acid; a violent action ensues, *nitric oxide* escapes in abundance, and the metal rapidly diminishes in quantity. You must not perform this experiment hastily, because the action is so strong that the materials will very likely fly out of the glass; but do it by degrees, as directed, and take care that there is *excess* of mercury remaining after the action is over. You will now find a quantity of a white crystalline substance in the glass, which is a *nitrate of oxide of mercury;* take it out by means of a wooden spatula, and put it into a small earthenware crucible, heat it gradually in the fire, and you will find fumes of nitric acid escaping. It is as well to stir the nitrate with an iron wire during this process, and when the heat approaches to dull redness, remove the crucible, and you will find in it a bright *red* substance, which is *oxide of mercury*, now liberated by the heat from its combination with nitric acid. If you continue the heat any longer, you will decompose the oxide, evolve its oxygen, and evaporate the mercury, and nothing will remain in the crucible; but stop short at the point mentioned, and when the oxide is cool remove it into a glass, and treat it with nitric acid; you will find that it dissolves, without any evolution of *nitric oxide*, because the mercury is already oxidized, and presents you with the *nitrate of oxide of mercury*.

In some cases, when nitric acid acts on a metal, the only result is *an oxide*, and *no* nitrate is formed: this is the case with tin, as you can *easily prove.*

Take a few pieces of tin-foil, or filings of grain tin, put them into a glass, and add a few drops of nitric acid; a violent action takes place, *nitric oxide* is abundantly evolved, and the tin is wholly converted into a *white* powder, namely, *peroxide of tin*, formed at the expense of the oxygen of the nitric acid, and which will not dissolve in any undecomposed part of the acid, to furnish a solution of *nitrate of oxide of tin.*

At a future part of our "Popular Course of Chemistry," when examining the properties of the metals, I shall endeavour to bring these matters more fully before you. I may, however, state, in conclusion of this part of my subject, that the *nitrates* are *all soluble in water*, and therefore they cannot be discovered by any precipitating agent.

From the experiments already detailed, you will readily perceive that nitric acid is a most powerful and valuable chemical agent; its discovery is by no means of recent date, for we find it mentioned by "Geber the Arab," who flourished in the eighth century, and very accurate directions for its preparation are given by Basil Valentine, in 1644; it was a favourite menstruum with the alchymists, who, in allusion to its intense solvent powers, named it "*aqua fortis*," a term even now in use, as already noticed.

Our modern chemical term of *nitric acid* simply implies that *nitrogen* is one of its constituent elements, and its composition was first demonstrated by the celebrated Mr. Cavendish.

Nitric acid is of vast importance in the chemical laboratory, and in many of the chemical arts: it is now prepared on an enormous scale, by the distillation of hundred-weights of nitre and sulphuric acid, and is sold at a very cheap rate.

We have now examined the properties of the *five* compounds which *nitrogen* forms with *oxygen*: it, however, forms some very curious compounds with other elements, and to these I shall direct your attention at a future opportunity.

INSTRUCTIONS FOR MAKING AND REGISTERING METEOROLOGICAL OBSERVATIONS.

[Continued from "Simultaneous Meteorology," p. 308.]

"*Of the Temperature of the Sea.*—The surface temperature of the water at sea should be registered, as a matter of course, with the same regularity and at the same hours as the barometer and thermometer. It is more conveniently (and with quite accuracy enough for the purpose) obtained by taking up a bucketful of the water and stirring round the thermometer in it. Whenever a change to the extent of 2° Fahr. appears to have taken place since the last observation, a fresh bucketful should be taken up, and the observation repeated. It should also be noticed whether rain has fallen since the last observation. A sudden depression of 3° or 4° indicates the near approach of land. In a voyage lately made from England to the Cape of Good Hope, the temperature of the surface water fell at once 9° Fahr., on approaching within a few miles of the entrance of Table Bay.

"The temperature of the sea at considerable depths can hardly be regarded as a subject of ordinary meteorological inquiry and regular registry, though undoubtedly one of much physical interest; for which reason it is not considered necessary to dwell further on it.

"*Of the Hygrometer, &c.*—In the absence of Daniell's Hygrometer, or of ether to cool it, the degree of dryness of the air may be ascertained by observing the temperatures marked by two thermometers suspended freely side by side (but not in contact) in the shade, and completely defended from all radiation *to* or *from* the *sky*, the one having its bulb and stem naked, the other with the bulb and lower part of the stem wrapped in linen or cotton, and thoroughly wetted with pure spring or rain water. The temperatures indicated by both should be noted when the wetted thermometer refuses to sink lower, and the conclusions left for subsequent calculations. The naked thermometer may be the 'External Thermometer' itself, in which case a coated thermometer may be kept always suspended near it, completely screened, as above mentioned, and wetted some minutes previous to the regular daily readings.

"If a hair hygrometer be used, its points of absolute moisture and dryness should be frequently ascertained, as they are apt to change. The former may be found by keeping it some time in a close covered jar lined with wet blotting paper, and having water in it, and noting the point of moisture beyond which it refuses to go. The latter, by keeping it in the same manner in a jar perfectly air-tight, over fresh-burnt quick-lime, till it refuses to indicate a higher degree of dryness.

"The best measure of the *momentary evaporating power* of the air seems to be the depression of the wetted thermometer below the dry one. But the *actual evaporation* from a given surface is quite another thing, and a question may very reasonably be raised, how far any useful approximation to a knowledge of the total evaporation from an extensive and diversified surface, unequally moistened, and variously exposed to the sun, defended by clouds, or refreshed by dews, *can* be obtained by any small or local experiments.

"The Rain-gauge is an instrument of such extremely easy construction, that any person who lives near a tinman can procure one. In arid climates, it must, however, be remembered, that it will often need examination and cleansing, owing to long intervals of disuse in which insects and dust may lodge. It will often happen, too, that the slight rain of one day, if left unregistered, may be entirely lost by evaporation in the next,—nay, that slight and transient showers may never enter it, being evaporated from it as they fall. The effect of copious dew, too, must be separated from that of rain, so that the mere registry of the contents of the gauge is not of itself a sufficient indication whether rain has fallen in the night or not. However, there are usually good reasons for decision on this point from other indications. Attention to the amount of dew is very necessary, not only because the meteorological questions involved are of a high degree of interest generally, but because in arid climates the dews are of almost as much importance to the maintenance of vegetation as the rain.

"*In stating* the quantity of rain daily received in the gauge, the *height of the* receiver above the soil should be mentioned, experience having shown that the quantities of rain which actually fall on a given area on the ground, and at a very moderate height above it, often differ materially. In some localities and circumstances, the rain-drops receive accession from the air as they descend, in others they undergo partial evaporation. The former is generally the case in cool moist climates,—the latter may be expected in this country.

"*Of the Wind.*—The points most important to remark respecting the wind, are,—

"1st. Its average intensity and general direction during the several portions of the day devoted to observation and registry.

"2ndly. The hours of the day or night, when it commences to blow from a calm, or subsides into one from a breeze.

"3rdly. The hours at which any remarkable changes of its direction take place.

"4thly. The course which it takes in veering, and the quarter in which it ultimately settles.

"5thly. The usual course of *periodical winds*, or such as remarkably prevail during certain seasons, with the law of their diurnal progress, both as to direction and intensity—at what hours, and by what degrees, they commence, attain their maximum, and subside, and through what points of the compass they run in so doing.

"6thly. The existence of crossing currents at different heights in the atmosphere, as indicated by the course of the clouds in different strata. In observing these, it is advisable to fix the eye by some immovable object, as some point of a tree or building, the sun, or the moon; otherwise mistakes are apt to arise.

"7thly. The times of setting-in of remarkably hot or cold winds,—the quarters from which they come, and their courses, as connected with the progressive changes in their temperature.

"8thly. The connexion of rainy, cloudy, or fair weather, with the quarter from which the wind blows, or has blown, for some time previous.

"9thly. The usual character of the winds as to moisture or dryness, not as deduced from mere opinion or vague estimation, but from actual observation of the hygrometric state of the atmosphere during their prevalence.

"Among these particulars it will be seen that some are of a nature susceptible of daily observation and registry, while others call for an exercise of the combining and inductive faculty on the observer's part, and cannot be made out otherwise than by continued attention and habitual notice of phenomena, with a view to the investigation of their laws. The general impression left upon the mind as to any of the points of this kind above enumerated, by the occurrences of the past month, will therefore be more properly stated, in the way of summary remarks at the end of monthly registers, than as entries under particular days.

"*Of the State of the Sky.*—In describing the state of the sky as to clouds, &c., the observer will bear in mind that it is only in that region of the sky which is vertically above him that the true forms and outlines of the clouds are exhibited, and the area they cover, as well as the intervals between them distinctly seen. As they approach the horizon in any direction, their extent is foreshortened by perspective, their apparent magnitude diminished by distance, and their intervals covered in, and hidden by their mutual interposition. In estimating, therefore, the quantity of clouds in the sky, regard must be had to this, and our judgment should rather be formed on a view of the region extending from the zenith every way half way down to the horizon, than from the aspect of the heavens below that limit. It would be better to notice both, and state, separately, the proportions in which each are covered, and the quarter of the horizon towards which the chief masses in the lower region lie.

"The general aspect of clouds, as classed under the heads Cumulus, Cirrus, Stratus, &c., should be noticed, and especially the height of this inferior surface, or the level of the *vapour-plane*, should be estimated. In a mountainous region this is easy, so long as the vapour-plane is below, or not far above, the summits of the hills, and in such regions the formation and dissipation of clouds in the neighbourhood of the mountain-summits, under the influence of certain winds, form a subject of study of a highly curious and interesting nature.

"The formation of clouds at night, during calm weather, under the influence of a gradually-descending temperature, is another point worthy of attention. It frequently happens that, without any perceptible wind, the sky will suddenly become hazy in some one point, and the haze condensing and spreading, in all directions, without a wind, the whole heaven will become overcast in a remarkably short time. The same thing will sometimes occur nearly at the same hour for many nights in succession. Such phenomena should be noted whenever they occur.

"*Of Thunder and Lightning, and of the Electrical state of the Air.*—Connected with this part of the subject is the observation of shooting-stars and luminous meteors. Remarkable ones should be noticed, and the moment of their appearance, their direction, duration, length of path, and *course among the stars*, ascertained and noted, with the phenomena of their increase and decay of light, apparent size, separation into parts, trains left behind, &c. The *general* direction (if any) which they observe on particular nights, is a point also to be attended to. When these splendid phenomena occur frequently and brilliantly in clear skies, there can be no doubt of their affording an available method of ascertaining the differences of longitude of the most distant stations, if duly observed by persons furnished with means of ascertaining the time.

"Thunder-storms, of course, will be noticed when they occur, under the general head of the weather, but it is of consequence also to notice distant lightning, not accompanied with thunder audible at the place of observation (by reason of its great distance*), especially if it takes place many days in succession, and to note the quarter of the horizon where it appears, and the extent it embraces. In an actual thunder-storm, especial notice should be taken of the quantity of rain that falls, and of the fits or intermittences of its fall, as corresponding, or not, to great bursts of lightning, as also of the direction of the wind, and the apparent progress of the storm with or against it.

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* "Thunder can scarcely ever be heard *more than 20 or 30* miles from the flash *which produces it.* Lightning, on the *other hand, may* be seen (or at least its reflection on the clouds, forming what is called sheet lightning,) at the distance of 150 or 200 miles."

"Observations of the electrical state of the air in serene weather are unfortunately too much neglected. The apparatus they require is simple, and by no means costly, and may be constructed, indeed, by any one for himself with ease.

"Attention to the accumulation of regular and daily observations of a definite and numerical character is, in the first instance, of the highest importance. In order that such observations may be registered with uniformity and ease, printed skeleton-forms should be prepared and distributed to all persons who may be willing to undertake their filling up. These may comprise only the registers of the barometer and its attached thermometer, with that of the external thermometer, and a column of remarks for wind and weather, as being the most essential and indispensable elements of meteorology; but it is in the power of any one who pleases to supply additional information, and to those who have leisure, instruments, and inclination for the task, it is particularly recommended to observe regularly the wet thermometer, the self-registering thermometer, and, weekly or monthly, thermometers buried at different and progressive depths beneath thesurface of the soil.

"The printed forms should provide for the arithmetical convenience of casting up the *means* for each month. In doing so, it is requested that care will be taken to verify the results by repetition, and (that usual sources of error may not escape notice,) it is recommended in every instance, before adding up the columns, to look down each to see that no obvious error of entry (as of an inch in the barometer, a very common error, or, what is more difficult of detection, an error in the first decimal place,) shall remain to vitiate the mean result. It is perhaps unnecessary to do more than mention the precaution of *counting* the days in *each* column on which observations occur, so as to admit of no mistake in the *divisor*, and to use throughout the decimal arithmetic in calculating the mean results. Care and exactness in these points will, in most cases, add greatly to the value of the registers, as it will be quite impracticable for any person, should observations accumulate in masses, unreduced or erroneously reduced, to undertake the overwhelming task of recomputing them.

"Although not, strictly speaking, a branch of meteorology, yet, as observations of the tides are now earnestly desired in every part that they visit, it would be especially desirable to obtain regular observations of the time and height of high and low water on the coasts, &c., according to the rules, and on the plan, proposed by Mr. Whewell, in his late researches on this subject."

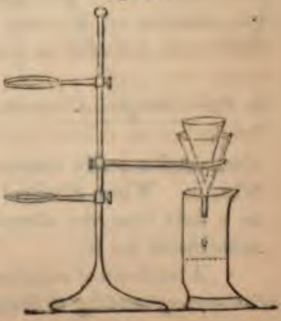
A POPULAR COURSE OF GEOLOGY.

VIII.

Chemical Characters of Minerals (*Continued*).

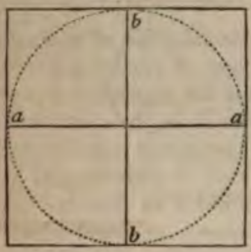
It will have been observed, that in the artificial classification of minerals described at page 204, the second order of the class of earthy minerals was characterized by their solubility in acids, while the characteristics of the two orders of saline minerals were made to depend on their solutions yielding or not yielding a precipitate with the carbonated alkalies; so that, under this classification, recourse must be had to experiments by the humid process, as well as by the blowpipe, in order to discover the name and composition of a mineral. It will, moreover, be found advantageous in conducting such investigations, that, in all cases, the results obtained by the pyrognostic examination of any substance should be confronted with those obtained by submitting another portion of it to the action of chemical tests, after having brought it into a state of solution. This may be done by means of the watch-glass and tube apparatus (fig. 29). The solution of the saline minerals in water, and of those that are soluble in acids, is easily effected. Compounds insoluble in acids must be reduced to fine powder, and fused with carbonate of potassa, or carbonate of soda. This fusion may be accomplished with the blowpipe, on platinum foil, or in the platinum spoon. The fused mass will then be soluble in water, and, portions of the solution being poured into several small tubes, may be tested with different reagents. Precipitation takes place when the reagent forms, with any of the earths or metals held in solution, a compound insoluble, or sparingly soluble, in the liquid in which they had been dissolved. The colours of the precipitates, and their degree of solubility in hot or cold water, or in acids, afford characteristics by which the different ingredients may be discriminated. Sometimes no precipitate is produced, but the colour of the solution is changed. In other cases, a small quantity of the reagent produces a precipitate which a larger quantity redissolves. It will be unnecessary to illustrate these cases by examples, as numerous instances will present themselves as we describe the action of reagents on some of the substances of most common occurrence in the mineral kingdom.

Fig. 32.



It will frequently be necessary, in conducting a qualitative analysis of this kind, to separate a precipitate by filtration, in order that the supernatant liquor may be tested for other substances remaining in solution. Filtration is performed thus: a piece of filtering paper, rather bibulous, and free from size, lime, and other impurities, is folded so as to fit a small glass funnel (fig. 32). The liquid, with the precipitate, is poured into the paper, the funnel being placed on a support over a small glass jar. The liquid will then soak *through the pores* of the filter, leaving the solid matter behind. The precipitate is to be washed on the filter, by repeatedly pouring distilled water upon it until the water which passes through the paper ceases to have any action upon test-papers stained with litmus or turmeric.

Fig. 33.



The filtering-paper is to be folded in the following manner, in order to make it fit the funnel. Double a square piece in half, in the direction *a a* (fig 33). Fold it again in the direction *b b*; cut off the corners, as shown by the dotted line, and then let the folded paper be opened by thrusting the finger into it, so that three folds of the paper may be on the one side and one on the other. A cup will thus be formed (fig. 34), into which, after it is placed in the funnel, the liquor may be poured. We have spoken of washing the precipitate with distilled water. The same should be used for making aqueous solutions, and for diluting acids in all analytical operations. The reason of this is, that spring water is never perfectly pure, but always contains some salts or earths in solution, which would yield a precipitate with some of the reagents, and give rise to erroneous conclusions. When heat is applied to water in which saline and earthy matter is dissolved, the water rises in the form of vapour, leaving the salt or earth behind, and, on condensing this vapour, the water is obtained in a state of purity. This is the process of distillation, which is conducted by means of the apparatus represented in fig. 35. A head of tinned iron, *a*, fitted to the vessel *b*, which contains the water to be distilled, is connected to a worm, *c*, passing through a vessel which is filled with cold water, in order to promote the condensation of the vapour, which trickles as it is condensed through the stopcock at *d*, into a vessel placed to receive it. This process should be conducted very slowly, in order to obtain the water in a state of perfect purity; for, should ebullition be allowed to take place, some of the foreign matter would rise, mechanically suspended in the vapour, and would be carried over with it. When distilled water cannot be procured, rain water, collected as it falls from the atmosphere, at a distance from any buildings, may be substituted for it.

Fig. 34.

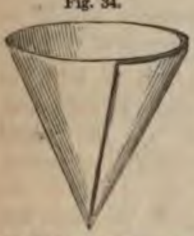
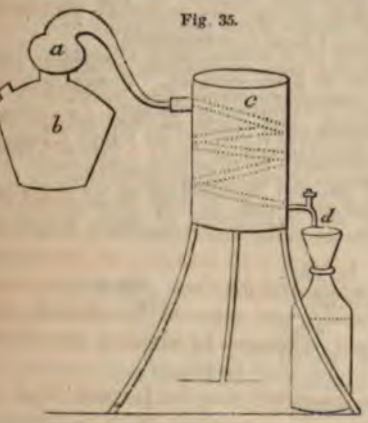
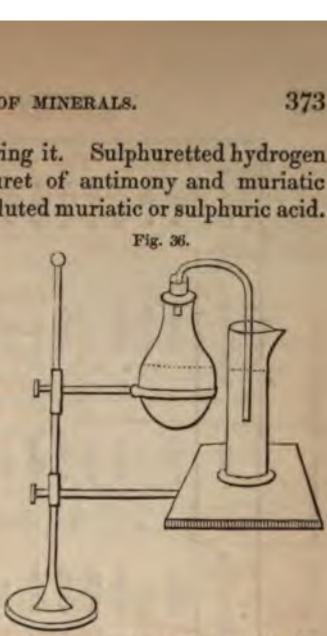


Fig. 35.



A current of sulphuretted hydrogen gas is a reagent of great importance *for separating* different metals contained in the same solution. We shall therefore describe the mode of preparing it. Sulphuretted hydrogen may be obtained either from protosulphuret of antimony and muriatic acid, or from protosulphuret of iron and diluted muriatic or sulphuric acid. In a glass flask (fig. 36), fitted with a cork having a bent glass tube passing through it, let sulphuret of antimony be heated by means of a lamp, with four or five times its weight of strong muriatic acid. An interchange takes place between the elements of the substances present, the composition of which is as follows. Aqueous muriatic acid consists of muriatic acid gas and water. The elements of water are, oxygen and hydrogen; of muriatic acid, hydrogen, and chlorine. Either the water of the acid, or the acid itself, is decomposed.

Fig. 36.



On the first supposition, the oxygen of the water unites with the antimony, producing the protoxide of that metal; and oxide of antimony, combining with the acid, forms muriate of antimony. On the second supposition, the chlorine of the acid forms a chloride with the antimony, and in both cases hydrogen is liberated, either from the water or the acid, which, uniting with the sulphur of the sulphuret, produces sulphuretted hydrogen. Sulphuretted hydrogen may also be procured from protosulphuret of iron, and sulphuric or muriatic acid diluted with three or four times its weight of water. In this case, the water is decomposed; its hydrogen unites with the sulphur of the sulphuret, and sulphate or muriate of antimony remains in solution. In this mode of preparing sulphuretted hydrogen, the application of heat is unnecessary.

Protosulphuret of iron may be obtained by igniting common iron pyrites, which is a deutosulphuret, so as to expel one proportion of sulphur, or it may be formed by mixing two parts of iron filings and rather more than one of sulphur, and exposing them to a low red heat, excluding, during the process, access of air as much as possible.

Bars or cylinders of iron, copper, zinc, and tin, will be required for precipitating different metals in the metallic state from their solutions, and some paper should be provided, stained with infusions of litmus and turmeric, as tests of the presence of acids and alkalies. The blue colour of the litmus paper is reddened by acids, and restored by alkalies, and the yellow colour of turmeric paper is changed by alkalies to reddish brown.

The reagents necessary to be procured may be learned from an inspection of the tables which follow.

A TABLE *of the Action of Reagents on Solutions of the Alkalies and principal Earths and their Salts in solution.*

	POTASSA.	SODA.	LITHIA.	BARYTA.	STRONTIA.	LIME.	MAGNESIA.	ALUMINA.
Pure Potassa . . .	—	—	—	[1] white	[1] white	[1] white	[1] white	[1] white
Pure Ammonia . . .	—	—	—	—	—	—	[2] white	[2] white
Carbonate of Potassa . .	—	—	—	[2] white	[1] white	[1] white	[3] white	white
Bicarbonate of Potassa . .	—	—	—	[3] white	[1] white	[1] white	[4] —	white
Carbonate of Soda . .	—	—	[1] slight	white	[1] white	[1] white	[3] white	white
Carbonate of Ammonia . .	—	—	—	white	[1] white	[1] white	[5] —	white
Hydrosulphuret of Ammonia	—	—	—	—	—	—	[6] —	[3] white
Liquid Sulphuretted Hydrogen	—	—	—	—	—	—	—	—
Sulphuretted Hydrogen Gas	—	—	—	—	—	—	—	—
Oxalic Acid	—	—	—	[4] white	[2] opalescent	[2] white	—	—
Oxalate of Ammonia . .	—	—	—	white	white	[2] white	[7] —	—
Tartaric Acid	[1] white	—	—	—	—	—	—	—
Chloride of Platinum . .	[2] yellow	—	[2] a troubling	—	—	—	—	—
Sulphuric Acid	—	[1] crystals	—	[5] white	[3] white	[3] white	[8] —	[5] crystals
Sulphate of Alumina . .	[3] crystals	—	—	[5] white	[3] white	[3] white	—	—
Phosphate of Soda . . .	—	—	[3] white	[6] white	[1] white	[1] white	[9] white	[4] white
Phosphate of Potassa . .	—	—	[4] white	[6] white	[1] white	[1] white	[9] white	[4] white
Prussiate of Potassa . .	—	—	—	—	—	—	—	—
Red Prussiate of Potassa .	—	—	—	—	—	—	—	—
Hydrofluosilic Acid . .	[4] gelatinous	[2] opalescent	—	[7] crystalline	—	—	—	—
Tinge the Blowpipe-flame .	[5] violet	yellow	carmine-red	yellow	carmine-red	red	—	—

We shall now describe the colours and appearances of the precipitates produced by different reagents, in solutions of the most common earths and metallic oxides. These will be rendered more striking by the preceding table. Under the name of each substance placed at the head of the column, we have given the colour of its precipitates, with the reagents, the names of which are placed on the left of the columns. Many of these precipitates require a fuller description than can be given in the columns: they have, therefore, been distinguished by numbers referring to paragraphs marked with corresponding numbers, in which, under the title of each substance placed at the head of the columns, will be found such explanatory particulars of its properties, and of the conditions necessary to the success of the experiment, as could not be admitted into the tabular form. Where a line hath been drawn across a column, it denotes that the substance at the top of the column forms no precipitate with that reagent.

POTASSA. Potassium 40 + Oxygen 8 = 48.

1. *Tartaric Acid.*—This precipitate consists of crystals of bitartrate of potassa. The tartaric acid must be in excess. The precipitation takes place immediately in concentrated solutions of potassa and its salts, but not until after some time in those that are dilute.

2. *Chloride of Platinum.*—A bright yellow crystalline precipitate of chloride of platinum and potassium. This is a very delicate test of the presence of potassa and its salts, if the mixture be evaporated to dryness, and a little cold water be afterwards added.

3. *Sulphate of Alumina.*—The solutions both of the potassa and the reagent must be concentrated, and crystals of alum (sulphate of alumina and potassa) will be produced, in the form of regular octohedrons, or segments of octohedrons. A solution of pure potassa must first be saturated with an acid.

4. *Hydrofluosilic Acid.*—This precipitate is scarcely perceptible in dilute solutions. It subsides very slowly, and is hardly less transparent than the supernatant liquor, and is chiefly discernible by producing an opalescent play of colours. In very concentrated solutions, a white troubling is immediately produced, the transparent gelatinous precipitate soon begins to subside, and no opalescence takes place.

5. The salt of potassa is to be fused on platinum wire at the point of the inner flame, when a violet-coloured tinge will be communicated to the outer flame. This colouring of the flame, however, is not exhibited by all the salts of potassa, and is more feeble in some than in others.

The best tests for potassa in solutions are chloride of platinum and tartaric acid. As chloride of platinum produces the same effects in solutions of ammonia as in those of potassa, it is necessary to ascertain the absence of ammonia before the presence of potassa can be determined. Ammonia is detected by its smell, and by giving out white fumes, when a glass rod, moistened with strong, but not fuming, muriatic acid is held over the liquid containing it

SODA. Sodium 24 + Oxygen 8 = 32.

1. *Sulphuric Acid.*—Crystals of sulphate of soda, or Glauber's salts, in four and six-sided prisms.

2. *Hydrofluosilic Acid.*—This precipitate is slight and opalescent, and is not produced at all in weak solutions.

Soda is distinguished from other alkaline bases by yielding crystals of sulphate of soda with sulphuric acid, by not being precipitated by any reagent, and by communicating a rich yellow colour to the blowpipe-flame, when fused on platinum wire.

LITHIA. Lithium 10 + Oxygen 8 = 18.

1. *Carbonate of Soda.*—No effect is produced until after some time, even in concentrated solutions. The inconsiderable, granular, and sparingly-soluble precipitate which then takes place, is carbonate of lithia.

2. *Chloride of Platinum.*—In spirituous solutions, a slight troubling, scarcely discernible.

3. *Phosphate of Soda.*—No precipitate, even after a long time, in the cold; but if the mixture be boiled, or if ammonia be added, a strong precipitate takes place.

4. *Phosphate of Potassa.*—No precipitate, even after the mixture has stood some time, or after boiling. The addition of ammonia, however, produces by degrees a strong precipitate.

Lithia is distinguished from the other alkalies by its greater neutralizing power,—by forming sparingly-soluble salts with carbonic and phosphoric acids,—by the chloride of lithium being highly deliquescent, and dissolving in strong alcohol,—by the alcoholic solution burning with a beautiful carmine-red flame,—and by all the salts of lithia communicating this red tinge to the flame of the blowpipe. When a salt of lithia is mixed with a salt of potassa, the red colour is produced even when the potassa is in excess; but when mixed with soda, the yellow flame of the soda prevails even when the lithia is in excess. Lithia is distinguished from the alkaline earths by forming soluble salts, and therefore yielding no precipitate with sulphuric and oxalic acids, and by its sparingly-soluble carbonate affording a solution which changes the yellow colour of turmeric paper to brown.

BARYTA. Barium 70 + Oxygen 8 = 78.

BARYTA, when pure, is of a grayish-white colour, and very friable. When moistened with water, it falls to powder, like lime, with the evolution of heat, and has an alkaline reáction on vegetable blue colours. With a larger quantity of water, it forms a crystalline mass, completely soluble in hot water. A concentrated solution deposits in cooling, the air being excluded, crystals of hydrate of baryta. This hydrate fuses very easily at a red heat, though pure baryta is infusible except at a much higher temperature. On exposing an aqueous solution of baryta to the atmosphere, it absorbs carbonic acid, and a white insoluble precipitate, the carbonate of baryta, subsides. It is, therefore, a good test for the presence of carbonic acid in gaseous mixtures.

1. *Pure Potassa.*—A voluminous precipitate, which disappears almost entirely on the addition of a considerable quantity of water, provided the potassa was quite free from carbonic acid. This solution affords, after a time, a white precipitate, by absorbing carbonic acid from the atmosphere.

2. *Carbonate of Potassa.*—A white precipitate of carbonate of baryta, soluble in free acids.

3. *Bicarbonate of Potassa.*—The same precipitate as the last, but with effervescence, in consequence of the disengagement of carbonic acid gas. This effervescence is in most cases the only difference between the effects produced by the carbonate and bicarbonate of potassa, when employed as reagents.

4. *Oxalic Acid.*—No precipitate, except in very concentrated solutions, and in those not for some time. The addition of pure ammonia causes a precipitate, except in very dilute solutions.

5. *Sulphuric Acid.*—The most diluted solutions of baryta give, with this reagent, a white precipitate, insoluble in free acids.

6. *Phosphate of Soda.*—Neutral phosphate of soda produces a white precipitate, not augmented by the addition of ammonia, and not soluble in free nitric or muriatic acid.

7. *Hydrofluosilic Acid.*—After a time a crystalline precipitate, nearly insoluble in free nitric or muriatic acid.

Baryta is distinguished from other substances thus:—It dissolves in water, and forms an alkaline solution. It yields a white precipitate with the alkaline carbonates, and it is precipitated by sulphuric acid and the soluble sulphates, as a white sulphate, insoluble both in acid and alkaline solutions. Its solution in muriatic acid forms, by evaporation, crystals in the form of four, six, or eight-sided tables, not soluble in alcohol, and which undergo no change on exposure to the air. Salts of baryta impart a yellow tinge to flame, but the blowpipe affords no certain criterion by which its presence can be detected. All its soluble salts are poisonous.

STRONTIA. Strontium 44 + Oxygen 8 = 52.

PURE strontia resembles baryta in appearance and infusibility, in possessing alkaline properties, and in slaking, when moistened with water, thus forming a hydrate, which fuses at a red heat, but is not decomposed by the most intense heat of a wind furnace. It is insoluble in alcohol, but soluble in boiling water, and a hot and concentrated solution deposits, on cooling, transparent crystals in the form of thin quadrangular tables.

1. *Pure Potassa.*—The behaviour of solutions of strontia with the pure and carbonated alkalies, and with phosphate of soda, is precisely the same as that of solutions of baryta.

2. *Oxalic Acid.*—In neutral solutions, a slight troubling. If they are very dilute, the opalescence does not take place for some time. The addition of ammonia causes an immediate precipitate.

3. *Sulphuric Acid.*—A white precipitate, insoluble in free acids.

Strontia is distinguished from baryta by forming, with muriatic acid, a salt which crystallizes in slender hexagonal prisms, deliquesces in a moist atmosphere, and dissolves freely in alcohol. This alcoholic solution burns with a bright carmine-red colour, and most of the salts of strontia impart the same colour to the flame of the blowpipe when fused on the platinum wire. It possesses this property in common with lithia, from which it is distinguished by giving a precipitate with carbonated alkalies. This red colour of its flame distinguishes it from baryta, from which it may also be known by giving no precipitate with hydrofluosilic acid, and by the greater solubility of its sulphate. Baryta is so completely precipitated from a solution of one of its soluble salts by an excess of sulphate of soda, that its presence cannot afterwards be detected by any reagent; but when a solution of a salt of strontia is treated with the same reagent, so much sulphate of strontia remains in solution, that the filtered liquor gives a white precipitate with carbonate of soda or potassa.

LIME. Calcium 20 + Oxygen 8 = 28.

PURE lime is white and friable. It has a great affinity for water, forming with it a bulky hydrate, with the evolution of great heat in the process of slaking. This hydrate is sparingly soluble in water, and more soluble in cold than in hot water. The solution has a caustic taste, changes the blue colour of infusion of violets to green, reddens litmus paper, and changes to brown the yellow colour of turmeric paper. It attracts carbonic acid from the air, with which it gradually forms a precipitate of carbonate of lime. Lime water is also rendered turbid by a solution of carbonic acid, but recovers its transparency on the addition of a larger quantity, because lime is soluble in an excess of carbonic acid. Pure lime, as also its hydrate, is infusible, except by the oxy-hydrogen blowpipe, and even by that with difficulty. It may, in fact, be considered one of the most infusible bodies in nature. It possesses, in common with baryta and strontia, the property of becoming highly luminous when exposed to a full red heat.

1. *The Pure and Carbonated Alkalies and Phosphate of Soda* produce the same precipitates in solutions of lime as in solutions of baryta and strontia.

2. *Oxalic Acid and Oxalate of Ammonia* both produce a white precipitate, even in very dilute solutions. The latter is a very delicate test for lime, by which it may be distinguished from most other substances, except baryta and strontia, both of which form sparingly-soluble oxalates. The oxalates of these three earths are soluble in water acidulated with muriatic acid.

3. *Sulphuric Acid, and Solutions of the Sulphates.*—The calcareous solution must be moderately concentrated. The most diluted solutions of baryta and strontia yield a precipitate with diluted sulphuric acid, and the precipitate is insoluble in nitric and muriatic acids. No precipitate of sulphate of lime takes place in diluted calcareous solutions, nor with diluted sulphuric acid, and the precipitated sulphate of lime is soluble in nitric acid. Lime is further distinguished from baryta and strontia by the following characters. Nitrate of lime crystallizes in prisms, is *very* deliquescent, and soluble in alcohol. The nitrates of baryta and strontia crystallize in regular octohedrons, or segments of octohedrons,

are not deliquescent, and do not dissolve in pure alcohol. Solutions of salts of baryta form a crystalline precipitate with hydrofluosilic acid; calcareous salts do not. Salts of lime, like those of baryta and strontia, are distinguished from the alkaline salts by yielding a precipitate with alkalies. The alcoholic solutions of salts of lime burn with a red flame, and some of its salts communicate the same colour to the blowpipe-flame, when fused on the platinum wire.

MAGNESIA. Magnesium 12 + Oxygen 8 = 20.

PURE magnesia is a white powder, without taste or odour, infusible, and nearly insoluble in water. Moistened, and laid on litmus paper, which has been reddened by an acid, it restores the blue colour, and it reddens turmeric paper, but does not change the colour of infusion of violets. Magnesia has a weaker affinity than lime for water, for when moistened, it gives out no heat, and the resulting hydrate is decomposed by a red heat.

1. *Pure Potassa.*—A solution of pure potassa produces, in magnesian solutions, if neutral, a white flocculent precipitate, which does not dissolve on diluting the liquid with water. This is one of the characters by which it is distinguished from baryta, strontia, and lime. Muriate of ammonia prevents this precipitate, and dissolves it when formed; but, under such circumstances, a precipitate takes place on boiling the mixture.

2. *Pure Ammonia.*—In neutral solutions a white and bulky precipitate, soluble in muriate of ammonia. This precipitate distinguishes solutions of magnesia from those of baryta, strontia, and lime, on which ammonia produces no effect.

3. *Carbonate of Potassa.*—In neutral solutions a voluminous precipitate, soluble on the addition of muriate of ammonia, which, if previously present, prevents a precipitate until the mixture has been boiled. No precipitate takes place with carbonate of potassa in magnesian solutions, containing much free acid, and which are not very concentrated, unless the mixture is boiled.

4. *Bicarbonate of Potassa.*—No precipitate in the cold, even in neutral and concentrated solutions.

5. *Carbonate of Ammonia.*—No precipitate in the cold. The precipitate produced by boiling dissolves in muriate of ammonia.

6. *Hydrosulphuret of Ammonia.*—When this reagent causes any precipitate in magnesian solutions, it is because the hydrosulphuret contains free ammonia.

7. *Oxalate of Ammonia.*—No precipitate is formed by this reagent in moderately-diluted solutions of magnesia. Magnesia may thus be distinguished and separated from lime.

8. *Sulphuric Acid.*—No precipitate. This property distinguishes magnesia from baryta and strontia.

9. *Phosphate of Soda* occasions a precipitate in neutral and concentrated solutions of magnesia. No precipitate is formed in the cold in dilute solutions, but a precipitate appears on the mixture being boiled, and does not redissolve as the liquid cools. Caustic ammonia and carbonate of ammonia cause an immediate precipitate in neutral magnesian

solutions which have been treated with phosphate of soda, when too much diluted to produce any precipitate in the cold.

Magnesia may be detected by the following properties. Its muriate and nitrate are very deliquescent, and are soluble in alcohol, and its sulphate is very soluble. Its neutral solutions are distinguished from solutions of alkaline salts by the white precipitate which they give with pure ammonia and carbonate of potassa, and from the alkaline earths, by giving a precipitate with pure ammonia. Its acid solutions are distinguished from those of potassa and soda, by yielding a white precipitate with a solution of phosphate of soda, after being supersaturated with ammonia, and from lithia, the only one of the alkalies which forms a sparingly-soluble compound with phosphoric acid, by giving a precipitate with an excess of potassa after being boiled. From a solution of baryta and strontia, its acid solutions are distinguished by affording no precipitate with dilute sulphuric acid; and from solutions of lime, by their behaviour with oxalate of ammonia.

Before the blowpipe, salts of magnesia may be detected by heating them on charcoal, moistening the assay with a solution of nitrate of cobalt, and again strongly heating it, when the mass will acquire a pale-red colour. Pure magnesia and the carbonate are to be made into a paste with water before they are ignited, and treated with a solution of nitrate of cobalt. The presence of metallic oxides, alkalies, and earths, (silica excepted,) prevents the production of this red colour.

ALUMINA. Aluminum 10 + Oxygen 8 = 18.

ALUMINA, in a state of purity, is insoluble in water, but soluble in acids, until after ignition, when it becomes quite insoluble in some, and nearly so in others. It is best dissolved after ignition by digestion in concentrated muriatic acid, slightly diluted with water. Its soluble compounds have no action on test papers. Alumina is infusible, though less so than lime and magnesia. Its powerful affinity for water has already been noticed.

1. *Pure Potassa.*—By this reagent alumina is precipitated as a bulky hydrate, which entirely redissolves in an excess of potassa. In this solution, a solution of muriate of ammonia produces a precipitate.

2. *Ammonia.*—A voluminous precipitate, insoluble in any excess of ammonia. The presence of muriate of ammonia does not prevent this precipitate, nor yet a precipitate with the carbonated alkalies.

3. *Hydrosulphuret of Ammonia.*—Sulphuretted hydrogen gas is set free, and pure alumina is precipitated, which is soluble in a solution of potassa.

4. *Phosphate of Soda.*—A voluminous precipitate, soluble in acids, and in a solution of potassa.

5. *Sulphuric Acid.*—If a concentrated solution, either of pure potassa or its carbonate, be mixed with a solution of alumina, and a slight excess of sulphuric acid be then added, crystals of alum will, after a time, be formed.

Alumina in solution is distinguished from solutions of the alkalies, *by giving* a white precipitate with the alkaline carbonates and caustic *ammonia*—from the alkaline earths, (except magnesia,) by its white precipitate with ammonia,—and from magnesia, by its precipitate with pure potassa,—being soluble in an excess of potassa, and insoluble in muriate of ammonia.

Before the blowpipe, alumina, and most of its compounds, may be detected by heating them on charcoal with nitrate of cobalt, in the manner described under magnesia, when a fine blue colour will be communicated to the assay.

Glucina, yttria, and zirconia, occur in such small quantities, and in so few minerals, and those so rare, that we shall pass them over, and proceed to describe the mode of detecting silica, which is one of the most abundant substances in nature, and we shall adopt that nomenclature which describes it as silicic acid, and its compounds as silicates. Silicic acid, then, as prepared artificially, is a white, gritty powder, without taste or smell, very infusible in the fire, and even by voltaic electricity, but before the oxy-hydrogen blowpipe it fuses more readily than either lime or magnesia. Native silicic acid, which occurs crystallized in rock crystals, and massive in common quartz and flint, is insoluble in all acids except the hydrofluoric, but is dissolved in a solution of potassa or soda.

Silicates which contain an excess of alkaline base are soluble in water, but become more insoluble in proportion to the quantity of silicic acid which they contain; and when they contain a great excess of it, are quite insoluble in water, and all acids, except the hydrofluoric. Glass may be mentioned as an example of a silicate insoluble from an excess of acid. The fused mass resulting from the ignition of a siliceous mineral with thrice its weight of a carbonated alkali, is a silicate soluble in water. When such a silicate is treated with concentrated muriatic acid, the silicic acid is precipitated as a jelly. If a large quantity of water be present, and if the acid be added gradually, the alkali may be perfectly neutralized, while all the silicic acid remains in solution. Partial evaporation of this solution produces a jelly, and on evaporating it to perfect dryness, and heating the residue with water, the silicic acid is rendered insoluble, and may be separated by filtration from the earths and metallic oxides with which it was combined, and which are redissolved on digesting the dried mass with water acidulated with muriatic acid.

There are some native silicates which, though insoluble in water, are soluble in concentrated muriatic acid, and they exhibit phenomena of two different kinds during decomposition. Some, when reduced to powder, and treated with cold muriatic acid, form a gelatinous mass, with disengagement of heat. On the addition of water, the bases previously combined with the silicic acid are dissolved in the state of metallic chlorides, and the free silicic acid remains in the form of delicate flocks. To this class belong the zeolites.

Other native silicates that are soluble in acids can only be decomposed by long digestion in muriatic acid, and produce no gelatinous mass. Native silicates which resist the action of acids are converted into alkaline silicates, soluble in water by fusion with carbonated alkalies. There are a few native silicates which cannot be decomposed either by acids or fusion with carbonated alkalies. These are decomposed by ignition with caustic potassa. In this process the crucible must be of silver instead of platinum, because pure potassa acts upon the latter metal.

From the list of native silicates soluble in muriatic acid, we select the following as the least rare, though none of them can be said to be very common: analcime, leucite, tabular spar, meerschaum, stilbite, sphene, and pitchblende. The following are the most common of those native silicates which cannot be decomposed, except by fusion with carbonate of potassa or soda: felspar, andalusite, olivine, prehnite, mica, chlorite, hornblende, augite, diallage, epidote, garnet, tourmaline, emerald, topaz, steatite, serpentine, pumice, obsidian, and pitchstone.

Zircon, cyanite, and cymophane, or chrysoberyl, are the only three native silicates which require for their decomposition ignition with caustic potassa.

Before the blowpipe, silicic acid alone, on charcoal or platinum, is infusible. It does not dissolve, or dissolves very sparingly, in microcosmic salt. In borax it dissolves completely, yet slowly, into a transparent glass. With soda it melts, with effervescence, into a colourless transparent bead of glass. As there are few substances which form a bead with soda, and none but silicic acid which form a colourless transparent glass, this is an excellent characteristic of silicic acid in the dry way, as its insolubility in all acids except hydrofluoric is by the humid process.

THE substances whose chemical characters we have now described are all of them, in a greater or less degree, essential ingredients of rocks. The same, as we have before observed, cannot be said of those metals (iron excepted) whose combinations with oxygen form neither alkalies nor earths. Nevertheless, there are few rocks in which some of them do not occur, in beds, in veins, or disseminated; and some are, in a great measure, peculiar to certain formations. It is, therefore, of importance to the geologist to be able to recognise these metals, and their compounds, when he meets with them; and this being the case, we shall proceed to describe the chemical characters of most of the metals, omitting the more rare, in the same manner that we have just described the behaviour of the alkalies, and of the most common of the earths, towards reagents.

Some of our readers may, perhaps, consider these details as dry and uninteresting, and may think that we are dwelling too much on the chemical and mineralogical part of our subject; but without some knowledge of mineralogy, it is in vain to think of studying geology; and it is a common complaint among those who have read some of the best treatises extant on that science, that they find themselves unable to profit by them, because of their ignorance of mineralogy, which prevents their attaching any definite ideas to the names of minerals and rocks which they find mentioned in those works: whereas the writers of most introductions to geology give their readers credit for a knowledge of minerals, or refer them, if ignorant of them, to living instructors and well-arranged collections. This is, without doubt, the easiest and quickest method of acquiring the requisite knowledge; and to those who may be within reach of such advantages, the details which we are now giving may be superfluous; yet even to those we would say, that, however well they may be able to recognise minerals by the eye, they have much to learn, if they know them by the eye only, and will derive great benefit from a chemical examination of their composition; while those students who reside in remote parts of the country, in the heart, perhaps, of a mineral district, but at a distance from lectures and cabinets, must be their own instructors, and must make the rocks around them their museum. We flatter ourselves that to such, this part of our work will not be uninteresting. The blowpipe and chemical tests must be their great resource. Practice and perseverance will soon render them expert in the use of them, and will engender a habit of examining the composition of every substance they meet with, which will prove a never-failing source of instruction and amusement. Perhaps it may be attended with other advantages, and may lead to fame and profit. It may be the means of sending down their names to posterity, as the discoverers of new simple bodies, yet unknown to chemists, which may be destined hereafter to play as important a part in medicine as iodine, or to be as useful to science and the arts as platinum. It may, perhaps, enable them to discover that some valuable ore, having a stony character, is used as a material for the repair of roads, or to detect a sufficient quantity of the precious metals in the waste of some mine, to render it no less valuable than the ore itself. It is not many years since the roads of Derbyshire were mended with an ore of zinc, through ignorance, and there are mines in Cornwall which have recently yielded a profit of several thousands of pounds by means of the silver extracted from the old heaps of refuse.

MISCELLANEA.

Dynamical Effect of Friction-Electricity.

M. Peltier having placed horizontally, *in the magnetic meridian*, a plate of copper about twenty inches long, and 2·4 in. wide, a thought struck him to rub it with a second copper-plate, which was only 2·4 in. square. One of the threads of a galvanometer touched the northern extremity of the larger plate; the other was attached to the smaller or rubbing-plate, which had a handle either of wood or of resin, in order that the hand which put it in motion should not warm it. All the rubbings were in one direction.

Twelve rubs, *directed from north to south*, produced in the needle of the galvanometer a deviation of about thirty degrees. The direction of the deviation evinced *negative* electricity.

Twelve rubs, *directed from south to north*, caused a deviation of about the same extent, but in the opposite direction, or that which would be given by *positive* electricity.

When the point of attachment of one thread of the galvanometer was carried from the *north to the south* of the larger or horizontal plane, the phenomenon preserved the same character; there was a difference in the amount, only, of the deviation.

Differences of quantity were also observed, according as the position of the point of attachment of the second thread on the second plate, permitted this point to follow or precede the rubbing-edge during the motion.

When the larger plate was in the plane of the magnetic equator, the rubbing had no effect.

It has been said, that the larger copper-plate was 2·4 in. broad; it was therefore practicable to place this broad face vertically, and still preserve the plane in the magnetic meridian. When in this position, the rubbing on the *western* face (whichever face otherwise it was) led always to a deviation sensibly larger than the rubbing on the *eastern* face.

It should also be added, that was it *not indifferent* whether, when the plate *lay horizontally*, it was rubbed on the *upper* or the *lower* surface. Rubbing on the *superior* face gave constantly a greater deviation of the needle of the galvanometer, than the rubbing on the face which was turned *downwards*.

The results given by the horizontal plate when it was exposed to the earth's magnetism only, were repeated nearly to the same amount of intensity, if the plate was placed upon a magnetized bar, with an intermediate body, such as a thin board.

A magnetized bar placed in the plane of the magnetic equator*, and on which a plate of soft iron is drawn, gives, as to the direction of the deviations, results perfectly similar to those of the horizontal plate of copper, if, as the theory requires, attention be paid to consider that end of the bar which is turned towards the *south* as the northern pole, and *vice-versâ*.

Substitute for Frogs in Galvanic Experiments.

Persons who may have occasion to repeat Galvani's experiment on the legs of frogs, will doubtless be pleased to hear of some substitute, which will enable them to dispense with the disgusting operations of cleaning, skinning, &c., which are necessary before the legs of a frog can be used. A leg of the common *grasshopper* may be made to exhibit the muscular contractions; and as it appears to be easily affected by electricity, can frequently be obtained when frogs cannot, can be prepared at a minute's notice, and retains its irritability for five or ten minutes, it forms an excellent substitute.

The method of preparation consists merely in removing, with a sharp penknife, from each side of the thick part of one of the leaping legs, a portion of the skin, so as to expose the flesh; then, by laying the under-side of the leg upon a small piece of moistened zinc, and bringing a piece of copper in contact with the flesh exposed on the upper-side, no motions will be observed until the copper also touches the zinc, when quick movements or jerks of the lower part of the leg will be seen, each time that contact is made.—Bailey. *Silliman's Journal.*

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* By adopting this position the complication which results from the influence of terrestrial magnetism is avoided.

Unfortunate Nomenclature.

At one of the meetings, during the present session, of the first scientific society in the "modern Athens," a paper was read, the principal subject of which was the production, and remarkable effects,—of waves in canals.

During the reading of this paper, and in the subsequent discussion and conversation, the following [phrases occurred, and were noted, as a curious specimen of cross-reading. Considering the precision and distinctness which generally prevails in scientific language, it would appear difficult to reconcile them with each other, and paradoxical to assert, that all of them were true; but such was the fact.

"The wave was a solid of least resistance,—was a mass of fluid particles,—had a fine entrance,—was large, solitary, progressive,—was of oak!

"The wave was constructed,—was propagated,—was built,—was generated in two ways, by the addition of a solid to a limited portion of quiescent fluid, and by the addition of a given quantity of fluid!

"The wave must not be confounded with the Dirleton, the Raith, and the Houston,—should be carefully distinguished from certain elevations on the surface of a fluid which may likewise be included under the generic title of wave. Observers who do not make this discrimination will be led into great confusion!

"The wave was made the subject of experiment at seven different degrees of immersion,—has had experiments made upon its age and history, its genesis and propagation, its route from the time and place of its generation, to the time and place of its observation!

"The wave was put in pretty rapid motion by a couple of horses, and then stopped, to allow the wave to move forward,—was attempted to be followed on foot, but its motion was found too rapid, and a horse was necessary to overtake it!

"The wave on the summit of a wave was found to be more easily directed than when the wave was behind a wave,—its lines of entrance were parabolic arcs, having a point of contrary flexure, between the maximum transverse section and the stem.

"The wave traversed space,—was dynamically trotted along the Union Canal,—was several miles long,—was 71 feet in length, builders'-measurement.

"There were, the ripple or dentate wave,—the wave that cost about 130*l.*,—the oscillatory wave,—the wave of six tons' burden,—the surge wave,—the wave of a very peculiar form,—the wave "par excellence,"—the wave projected at an angle of the line of vision sin. $-1 = \frac{1}{3}$!"

The key to the preceding enigmatical statement is,—that of several boats used in the experiments upon the *Canal-wave*, one of them, built for the occasion, and the favourite of the author of the paper, was most unfortunately named "The Wave!"

Question to Sir John Herschel.

"The proposition of Sir John Herschel for submitting the whole atmosphere of the globe to periodical examination at the same moment, has, from the vast extent of the operation intended, an air of philosophic grandeur.

"The attention to religious scruples, of which there is evidence in the same paper, is also conceived in the best spirit, and combines most harmoniously. I have felt it so deeply, as to hesitate whether I should be accessary, in the most remote degree, to disturb the sentiment; and I am induced to do it only by the consideration that it must, and will, happen, and that the earlier it is done probably the better.

"The instructions contain the following direction:—'Should any one of those 21st days fall on Sunday, then it will be understood that the observations are to be deferred till the next day, the 22nd. The observation at each station should commence at six o'clock A.M. of the appointed days, and terminate at six o'clock P.M. of the days following, according to the usual reckoning of time at the place.' It is clear that Sir J. Herschel, when he wrote this passage, unconsciously considered, that there were no observers upon the earth but Christians, and that if he provided against any violation of their observance of Sunday, he had secured his project from the deficiency which the conscientious abstaining from observing on such Sundays must produce. Perhaps, if the fact had occurred to his memory, that the Christians form, at present a minority in the world, he might still have decided as he has done, from the consideration that in this minority there lay the immense majority of observers: and it is not this selection, however injurious, which we wish to bring under observation. It is to the remarkable inadvertence of assuming that the provision in the terms stated accomplishes the object, namely, that of rendering it unnecessary to employ any part of the Sunday in the observations suggested. Do ask Sir John, *What will be the effect 'should any one of those 21st days fall on' Saturday?* We need not wait for an answer from Feldhausen to see that every hour of the 'day following'—*Sunday*—up to 6 o'clock in the evening of that day, will require an observation!

"I am gratified to find, upon examination, that no 21st falls either on Saturday or Sunday in the present year, and therefore Sir John has sufficient time to warn meteorological observers of any change he may think proper to advise."—CORR.

New Standing Order for the Protection of Walking.

THERE is a destruction of private rights and individual enjoyment now daily perpetrating, which is disgraceful to the legislature which permits it. We mean the annihilation of the ancient field-footpaths by railways. The evil was tolerable in the case of canals, though just as injurious as far as it was carried; but their number was comparatively small, and they introduced water into the view, which is always pleasant, and there was, on one side at least, a path which took you to the nearest bridge, and you were thus saved the disappointment and vexation of returning upon your own steps; but these railways seem prepared for the path of the thunderbolt, where they sink beneath the surface,—the chasm left by an earthquake is not more impassable where they rise above it,—Mont Blanc might as well lie in your path; they excite no idea to the lover of nature and the inhabitant of the country but interruption, and separation, and danger, and the hissing and withering approach of Mammon and Moloch*. The *circumbendibus* of all our

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* To a large portion of our readers this may seem an exaggeration. Those who are conversant with the subject know otherwise, and the Committee-rooms of the Two Houses of Parliament daily present instances which justify the assertion. We give one which occurred in the last month. Newliston, the residence and estate of a gentleman in the south of Scotland, lay in the route of a proposed railway. The intentions of the promoters of this railway with regard to this estate were of the most murderous nature. Among other witnesses to prove the mischief that threatened it, the owner introduced one who bore a name the most prepossessing that could be imagined, a name that will be for ever memorable in the annals of rapid travelling, (the professed object of all railways,) but equally dear to the recollection of every lover of the picturesque in nature. On the 2nd of May last—

MR. R. S. GILPIN

appeared, and gave the following evidence before a Committee of the House of Commons, of which Lord William Bentinck was Chairman. The reader will find the chasm is literally there†.

"You are a landscape gardener? Yes.—And have been so for many years? Yes.—Nearly twenty years, I believe? More, if I take it from my own determination, and nothing else.—You have been in the habit of suggesting improvements for ornamenting the properties and residences of many noblemen and gentlemen for a great part of that time? For all that time, and for some hundreds."

"Now, Mr. Gilpin, I believe the direction in which the railway is to cross this property (*Newliston, near Linlithgow,*) has been pointed out to you? Yes, I have seen the plan and drawing.—Have you seen the position in which it cuts across that avenue to the west, and severs the lodge from the house? Yes, *it draws right between the one and the other, and cuts off the approach to the house; it is impossible to see it.*—Now, looking at it as you are accustomed to view properties of this nature, with a view to make a much more extensive approach,

† The *Alp* will be found in the following answers of Mr. Horne, a surveyor.

"What is the height at the point where it (an embankment) enters Newliston? *Sixty-three feet I think is the height of the viaduct, and some of the embankment will be nearly sixty feet in height at the turnpike road.*—What is the length of the viaduct before you come to the embankment? *Two thousand feet of viaduct.*—Not upon this one property alone? Yes; *upwards of two thousand feet.*"

great towns will soon be impossible to everything but birds and balloons. When London shall be as complete as the disinterested attentions of railway-makers appear to promise, the access of the rural districts to each other will be most seriously injured. Birmingham may be brought nearer the metropolis, but Merton will have been far removed from Wimbledon. Places once so near, that a resident in one might summon a midwife from the other while the church-clock was striking eleven, are now miles asunder! Their intercourse is diminished, if not altogether suspended; neighbours are becoming foreigners, and their communication is by errand-carts and the Three-penny Post,—there is every reason to fear that it must eventually be the General!

Will the two Houses of Parliament extend their protection to foot-paths, to field-roads, and to short cuts? Will they consider the pedestrian, the cottager, the labourer, the villager, the poor, the aged, the lover of fields, and the shunner of turnpike-roads, by passing one inexorable standing order?

"NO RAILWAY SHALL DESTROY, OR RENDER DANGEROUS, ANY EXISTING FOOT-PATH."

It is not asked, that it shall not yield somewhat to the invader, that it may not be obliged to go over, or under, nor turn aside, a certain distance. Such unreasonable requests are not dreamt of. It is only desired that there may not be taken away from the helpless that which can be spared, and that the too-prevailing spirit of exclusion from the fields, and the woods, and the hills, which so highly and disadvantageously contrasts our provincial aristocracy from that of Germany, may not be universally dominant over the country-side, making turf and moss and green sward, as strange to the foot of the rambler in England, as it is to that of him who happens to be speeding from Cairo to the Great Pyramid.

could a railway there be at all tolerated? It is utterly impossible,—at least no gentleman could live there if he had an eye. If he were blind it would be another thing. —Would you not suppose, in your judgment, that he would have a chance of being incommoded, even if he had an ear? Yes, and his nose might occasionally.—But, without really speaking of the thing as a matter of ornamental property or as a private residence, severing the lodge from the house, and cutting through the avenue, would it be the destruction of the place? *The utter destruction of the place—it would be impossible that a gentleman could reside on this property—he could not live there. You might as well take a road through his drawing-room.*—I suppose you are a person of such great experience in landscape-gardening, that railways would never enter into your contemplation at all? No.—You think them utterly execrable? Indeed I do, utterly execrable."

"As an admirer of the picturesque, do you think *a trench, thirty feet deep, cut four miles through anybody's property*, would be an improvement to it? I think I need hardly be asked such a question by any person.—Is not such a sort of thing likely enough to horrify an admirer of the picturesque? *I cannot conceive that anybody would propose such a thing, who had common Christian feeling and good nature.*—You have seen, Mr. Gilpin, the line of canal there, which is a qualified nuisance, as a winding canal might be planted out and made ornamental? A good deal of it might. —But what should you think of a large red strip like this *(exhibiting the plan to the witness)*, running by the side of a canal? I should hope really that nobody would propose to do such a thing as that.—It is miraculous; but hard-hearted engineers, and cruel and flinty projectors of railroads, and the getters-up of such schemes, have not eyes for the picturesque? How is it possible that a gentleman could live on an estate with a trench of such a depth running through it!—What should you think of a magnificent property, having a railroad by the side of the canal? It is utter ruin, for if the thing failed, there would be the ditch for everlasting; it is too bad, *you could never get anything to compensate you for it*—it is too bad.—I do not want to detain you very long, but I must ask you this question: must not a man who would project such a line as that, have a heart as hard as a railway? There is one thing certain, he cannot perform his duty towards his neighbour; *he cannot be a Christian, that is impossible.*"

New honours to the name of Gilpin! We are gratified to see it has descended upon one so worthy. Still, notwithstanding his noble stand against the merciless intruders, we fear the fate of Newliston is pronounced. The Committee reported in favour of the promoters, (and this must be some satisfaction to Mr. Gilpin,) but only by a majority of one. Ought such a majority to be sufficient?

Steam Engine Duty; Cornwall against London and all England.

The extraordinary care, and the ingenious means to obtain accurate accounts of the work done by the steam-engines of Cornwall, are producing the most valuable results. They are daily accumulating data of the most useful character. Till these were furnished, and under unimpeachable circumstances, the steam-engine makers and the steam-engine workers of the rest of the kingdom, particularly of London, were wont to give the statements of the Cornish Engine-Duty a flat denial, treating them not as simple exaggerations, but as wilful falsehoods. Even within the month which has just expired, a member of the Institution of Civil Engineers, at a meeting of that body, stood up to prove that the officially-recorded effects were impossibilities, and, therefore, "could not be," and that all the vigilance of sworn and *disinterested* inspection, and of experiment and *interested* rivalry, had been deceived up to the present moment. It is but just to say, that, in general, the London steam-engineers have given up this mode of defence, and are seriously examining into the cause of their enormous inferiority to those of Cornwall. We present them with two Tables, drawn from sources which, from the public spirit of the Cornishmen, are provided for the inspection of all. At the present moment there is not the smallest similar co-operation among the Londoners to obtain the necessary information, and, consequently, they have no means of making such accurate comparisons, and obtaining such trustworthy results as their countrymen in Cornwall now enjoy. They know just enough to check their old scepticism, and improve their manners; and they have just courage enough to deny, though they have no evidence to prove the contrary, that the best of their engines, with the same quantity of fuel, scarcely exceeds in performance one-third of that of an engine put up in Cornwall within these two years. We understand that it would probably be impossible to obtain a register that could bear examination, of the consumption, vaporization, and effect of the boilers, of any engine on the shores of the Thames, during a period of any length.

TABLE I.

A Statement of the quantity of Water injected into the Boilers, and the quantity vaporized by the combustion of a Cornwall Bushel of Coals (94 lbs.) at Loam's Engine, United Mines, Cornwall.

(The consumption of Coal was ascertained by Mr. Thomas Lean, Sworn Registrar of the Duty of Steam-Engines in Cornwall, and is taken from his published Register.
The quantity of Water injected and evaporated was determined by the use of Hocking and Loam's Water-Meter, an instrument which carried off the first prize in this class, at the Anniversary of the Cornwall Polytechnic Society, in September last.)

DATE.	TIME.	COALS.		WATER.			REMARKS.
1836.	Number of Days.	Consumed.		Injected into Boiler.	Vaporized per common Bush. of Coals (94 lbs.)		
PERIODS.		Cornwall Bushels, (94 lbs.)	Imperial lbs.	Imperial Gallon.	Imperial Gallons.	Cubic Feet.	
March 2 to March 31	30	2417	227,198	223·020	92·27	14·8	
March 31 to April 30	30	2663	250,322	308·245	—	—	Boiler leaky
April 30 to June 3....	36	2741	257,654	252·105	91·8	14·7	
June 3 to July 2.......	29	1988	186,872	194·880	98·0	15·6	
July 2 to August 5...	36	2107	198,058	220·080	104·4	16·7	
1	2	3	4	5	6	7	8

A comparison of Columns 4 and 5 shows that the gallons of water injected are very *nearly equal to* the lbs. of coal consumed; so that in an approximative estimate it may *be taken, that, to* vaporize a gallon of water, the combustion of a lb. of coal is required.

TABLE II.

A Statement of the *greatest* and *least* quantities of Water, in *Imperial gallons*, discharged per minute from the Mines in Cornwall in 1836; specifying the Months in which the discharge reached its *maximum* and *minimum*.

MINES.	MAXIMUM.		MINIMUM.	
Wheal Unity Wood	April	466,38	October	235,26
Poldice	...	665,34	...	302,49
Wheal Damsel	February	71,26	November	59,45
Wheal Jewell	December	105,74	October	53,18
Cardrew	April	354,32	November	105,68
Wheal Tolgus	...	915,16	October	609,39
Dolcoath	December	326,52	...	180,34
East Wheal Crofty	April	212,2	October	102,83
North Roskear	December	199,1	...	135,17
South Roskear	April	153,67	November	111,67
Tincroft	December	198,4	September	114,86
Binner Downs	January	1328,59	October	775,79
Wheal Darlington	April	1115,3	November	830,93
Marazion Mines, *Powlet's*	February	503,54	...	273,47
Ditto *Rodney*	December	93,95	—	—
Consols	March	1546,4	October	1344,97
United Mines	May	1375,85	...	975,75
Wheal Beauchamp	December	341,9	September	192,02
Polgooth	April	1212,81	August	608,42
Pembroke	...	724,08	October	458,04
East Crinnis	April	991,38	October	515,32
Fowey Consols, *Austen's*	December	410,03	January	276,66
Ditto *Sawle's*	April	97,2	October	53,76
Roche Rock	December	286,13	...	180,01
Bryn Tye	...	30,73	...	—
East Wheal Rose	September	180,56	January	95,87
Wheal Vor	December	1010,49	—	—
Great Work	...	420,1	—	—
Wheal Leisure	February	1044,64	September	438,22
Great St. George	March	778,2	November	577,11
Wheal Prudence	April	148,24	October	110,76
South Towan	March	277,97	June	207,53
Relistian	December	164,94	January	136,22
Wheal Prospect	May	126,29	September	66,17
Wheal Virgin	April	266,63	...	207,55
Wheal Leeds	December	208,78	October	135,96
Ballaswidden	...	18,31	June	8,33
Levant	February	35,4	September	26,0
Ding-Dong	December	51,53	August	17,37
Providence	September	156,98	January	120,4

LEAN. *Report of Steam-Engine-Duty, December,* 1836.

French Manufacture of India-Rubber Web.

"The fabric of India-Rubber Web was commenced at Vienna, but much improved and extended in the manufactory at St. Denis, near Paris, in which there are about 1500 of the machines for plaiting the thread around the filaments of the elastic gum, and all the other departments in correspondent proportion.

"1st operation:—The gum-elastic is provided in the usual form of bottles. The first operation is to divide these bottles into two equal parts; they are then placed in piles of six or eight in height, and of an indefinite number in extent, upon a plank, and another plank is placed upon them, when the two are drawn together by wooden screws and nuts. They remain in this state a sufficient time to render them flat, or to take out in a great measure the original curvature of the bottles.

"2nd operation.—The first machine contains a circular knife which revolves rapidly, its diameter being about eight inches. At the side of its edge is an advancing carriage or slide, which receives its movement by means of a screw from the shaft of the knife. Upon this slide is attached the gum, a hole being made in its centre to receive a screw, which serves as a pivot upon which it may turn; it is held down by a nut that is screwed upon it, and the edges are held down by springs placed near to the knife, but not so strong as to prevent its turning under them. A box under the table contains water, in which the knife runs, and a box above it encloses the blade and prevents the water from being thrown into the face of the workman When the machine is started, the gum advances and is turned round by hand, whilst the knife cuts off the irregular circumference, until a continuous slip comes off, which the workman takes hold of and draws away, the carriage advancing, and the knife, cutting until the gum is exhausted. The operation resembles the cutting of leather strings out of circular pieces of that material in the manner practised in the olden time by shoemakers.

"3rd operation.—These slips pass into a bucket of water, from which they are taken and examined through their whole length by a woman, who removes the defective parts, and joins together the ends of the slips, by cutting them off in a sloping direction, and making a nick near the extremities, with a pair of scissors. These ends are then placed together and hammered with some force upon an anvil, by which means they are made to adhere with considerable tenacity.

"4th operation.—These slips thus joined pass to another engine, which resembles in almost all respects the slitting mills of iron works, of a size proportionate to the material upon which they operate. The slip, always contained in water, is guided into this cutting mill, which has five or six blades according to the width of the slip, and is kept in its place and prevented from turning by a slight spring. After passing between the cutters it is drawn off by two rollers, between which it passes, and from thence into the hands of the attendant, who passes the slip, thus divided into threads, into water.

"5th operation.—The filaments then pass into the hands of females, who examine them through their whole extent, remove the imperfect parts and join the extremities as before.

"6th operation.—The next machine is important, having for its object to remove the elasticity of the gum, or in other words, to stretch the filaments to their utmost extent. It consists of a reel of eighteen or twenty inches in diameter, revolving with considerable rapidity. Between the attendant and the reel is a wheel with several grooves of different diameters, revolving with a movement slow compared to that of the reel, and which has a transverse movement from the right to the left side, thus serving as a guide to the filament, and preventing it from overlapping upon the reel. This latter wheel was evidently intended to give an equal tension to the gum, as it was wound upon the reel, but the filament was simply held by the hand, and the wheel only used as a guide; sufficient practice on the part of the workman giving to the motion every desirable regularity. The slips are left upon these reels to dry and harden for a period varying from three to six weeks.

"7th operation.—They are then wound upon bobbins by the usual means of a wheel and spindle, by a woman, care being taken to retain the tension.

"8th operation.—The next operation is the plaiting of silk, cotton, thread, or other material, around the filament of gum, previously coloured or white, according to the objects into which it is subsequently to be manufactured. This is performed by an extremely ingenious machine, the construction of which it would be impossible to illustrate without drawings; the machines are manufactured, and for sale in Paris, by Blanchin, No. 98, Rue Faubourg, St. Martin. They have the important quality of stopping, if a thread breaks or is exhausted.

"9th operation.—The machine last alluded to draws the filament off the bobbins upon which it was previously wound, and after plaiting around it, winds it again upon others, which when filled, are conveyed to the looms, and there placed in frames, with a strap and counter-weight to give the necessary tension, and in sufficient number to form the warp of the web, which of course varies in width according to the object to which it is destined. The looms were usually simple and moved by hand, but there are also looms capable of weaving six webs or more at the same time, the shuttles of which are furnished with racks, by means of which they are carried through the chain.

"The plaited filament is combined with silk or other matter, and filled with different materials, according to the object of the manufacturer, and in this respect, all the variety of the weaver's art may be exercised.

"All the operations thus far noticed, have been performed by machinery, driven by a steam-engine, with the exception of the looms, which it appears to me are not necessarily excepted. In most of them the gum has been deprived of its elasticity, the last operation consists in restoring this quality. This is effected by taking advantage of that well-known though extraordinary character which gum elastic possesses, of shrinking by the application of heat.

"10th operation.—The machine to effect this is a long table covered with coarse cloth or felting in several thicknesses; at each end is a shaft crossing it from one side to the other, upon which are pulleys,—a strap passes over these pulleys, connecting the two ends of the table by a band, which has upon it a crotch. One of the shafts is furnished with a handle to give motion to the whole. A heavy, square, smooth iron, heated to a convenient degree, is drawn by means of these straps from end to end; three or four webs are laid upon the table at one time, their extremities on the right are held by weights, whilst a light block lies upon them at the other extremity, keeping them flat, but not preventing their advancing as they shrink by the application of the heat of the iron; inclined planes near the ends lift off the weight at the close of the operation. The iron has wooden handles for convenient management. Baskets at one end, and boxes at the other, receive and supply the web.

"The web shrinks in length as the heated iron passes over it, to about ⅔ of the previous length, and has all its original elasticity restored. This operation closes the process, the web being subsequently prepared for sale by being made into rolls, and properly packed."
—PEALE, *Franklin Inst. Journ.*

Marking-Ink not indelible.

"MARKING-INK may be removed from linen by a preparation of ammonia; I have made some experiments on the subject, and the result warrants me in asserting that it can be entirely obliterated by means of "Liq. ammoniæ, *fortissime.*" BENTLEY, *Lancet, May*, 1837.

Premiums and Prizes, for 1837, *offered by the Royal Cornwall Polytechnic Society; not confined to the County.*

1. For the best reports of a series of experiments made with the wedge for blasting rocks, invented by R. W. Fox, Esq., described and figured in the last Report of the Society . . *Ten Guineas.*

Offered by Sir Chas. Lemon, Bt.,
and R. W. Fox, Esq.

2. For the best chemical or mechanical plans for ventilating mines, which can be applied to the Cornish mines with advantage . . . . *Seven Guineas.*

3. For the second best ditto. *Three Guineas.*

Both by G. S. Borlase, Esq.

4. For the best essay on the various diseases incidental to miners, their causes, and the best practical means of remedying them. Any statistical information as to their longevity, compared with that of the other population of the county, will be deemed highly desirable. *Ten Guineas.*

By G. C. Fox, Esq.

5. For the best available method, or improvement on the plans already sug-

gested, for facilitating the ascent and descent of miners, provided the judges shall consider it to possess sufficient merit to be entitled to the premium. *Ten Pounds.*

By John Hearle Tremayne, Esq.

6. For the most complete and accurate accounts of the quantity of water supplied to the boilers, the number of bushels of coals consumed, and the duty performed by an engine, for a period of not less than six months in the ensuing year. . . . . *Three Guineas.*

By John Taylor, Esq.

7. For the most economical, safe, and efficient plan for lighting mines, consistent with the health of the miner: such plan is to be accompanied by a statement of the present actual consumption of candles, and the cost per dozen lbs., at some of the principal Cornish mines. . . . . . . *Ten Pounds.*

By the Rev. Canon Rogers.

8. For the best practical plan for adapting to steam-vessels the method used in Cornwall, of working steam expansively; including practical drawings of the construction of the boilers and expansion-gear. Such boilers should combine economy of fuel with safety, both as regards the danger from explosion, and accidents to the vessel from fire; with suggestions as to the best method of preventing the loss of heat, by radiation or otherwise. Due regard must be had to the essential difference between a single-acting engine working pumps by a lever, and two double-acting engines working cranks. . *Ten Pounds.*

9. For the second best ditto. *Five Pounds.*

Both by H. H. Price, Esq.

10. For the best model (either original or copy) not less than eighteen inches in length, of a life boat, which shall be judged most manageable in a storm. *Economy in the construction is a great desideratum.* . . . . . *Three Guineas.*

11. For the second best ditto. *Two Guineas.*

12. For the third best ditto. *One Guinea.*

All by Charles Fox, Esq.

13. For the best description and drawings of the least inconvenient and *inexpensive*, and, at the same time, *most efficient* means of securing a *fortnight's supply* of bread and water, within reach of a ship's crew, in the event of their not being able to go below deck, owing to the vessel being water-logged, or to other causes: the number of the crew may be estimated in the proportion of fifteen tons register to each man. . . . . *Three Guineas.*

14. For the second best ditto. *Two Guineas.*

15. For the third best ditto. *One Guinea.*

All by Charles Fox, Esq.

All plans should be accompanied with accurate models or drawings, estimates of expense, and every information necessary to enable the judges appointed for the purpose, to form a correct judgment of their respective merits.

Should several plans for any premium be proposed by the same person, a model of one, with accurate drawings and estimates of the others, will be sufficient: no individual to be entitled to more than one premium, where two are offered for the same object.

The judges will be requested to withhold any of these premiums, provided no plans be brought forward, which they shall deem of sufficient importance to merit them; the premiums will then be continued to another year.

Competitors for the foregoing premiums are requested to send their plans, &c., *free of expense*, to the Secretaries, Mr. Lovell Squire, and Mr. Thomas B. Jordan, *Falmouth*, one month before the next exhibition*, of which due notice will be given.

Lander Prizes

In commemoration of those enterprising travellers, Richard and John Lander.

16. For the neatest and most correct maps of some one state, province, or European Colony, comprising not less than 144 square miles; or a portion of not less than 100 square degrees of some uncivilized region . . *Two Pounds.*

17. Second best do. *One Pound.*

18. Third best do. *Twelve Shillings.*

19. Fourth best do. *Eight Shillings.*

The principal rivers, lakes, chains of mountains, line of coast (if any,) and territorial line, should be accurately delineated; and the size of the most important cities, or towns, with their latitudes, and longitudes, should be correctly marked. The maps should be accompanied by the fullest practical information (with reference to authority), either respecting the principal river flowing through the country, selected, as it relates to the length of its course, breadth at different places, tributary streams, lakes and canals, the periodical rise, average fall per mile, and rapidity of its current; the progressive increase of alluvial deposit in its formation, and the obstructions which may be opposed to its navigation; or respecting the physical characters of the principal chain of mountains in such country, their general direction, respective heights, geological features, more important passes, limit of perpetual snow, and the elevations at which various trees and plants will flourish on their sides.

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* The last exhibition was held on the 6th, 7th, and 8th of September, 1836.

By Charles Fox, Esq., annually, so long as he continues a Member of the Society.

A few Prizes will be given for useful Inventions and Improvements, sent to the exhibition, by persons residing *out of the county.*—*Report of Cornwall Polytechnic Society*, 1836.

Officer very much wanted in British Patent Offices.

THE following portrait of an "official personage" is not a figment of the brain. His residence is not in Utopia. He is flesh and blood, sitting in his office at Washington. So highly esteemed is he, that the General Government, after a very short acquaintance, are about to increase and multiply the breed. We wish they would make one more than they want, and permit us to "swop" our Attorney-General—He of the Hanaper—and some five or six other useless fee-takers which infest our patent offices, for the precious individual.

"It is his business to make himself fully acquainted with the principles of the invention for which a patent is sought, and to make a thorough investigation of all that has been before known or invented, either in Europe or America, on the particular subject presented for his examination. He must ascertain how far the invention interferes in any of its parts with other previous inventions, or things previously in use. He must point out and describe the extent of such collision and interference, that the applicant may have the benefit of the information in so shaping or restricting his claim of originality as not to trespass upon the rights of others. The applicant should also be referred to the sources of this information, that he may be able to satisfy himself on the particular points of interference. This frequently leads to a lengthy correspondence, before the applicant can be persuaded that his invention, or some rejected part of it, is not new. He often employs skilful and persevering counsel to urge and enforce by argument new views of the principles of his invention, who sometimes brings to his aid much mechanical astuteness. The examiner must also see that the specification accords with the drawing, and that the model is in conformity with both.

"An efficient and just discharge of these duties, it is obvious, requires extensive scientific attainments, and a general knowledge of the arts, manufactures, and the mechanism used in every branch of business in which improvements are sought to be patented, and of the principles embraced in the ten thousand inventions patented in the United States, and of the thirty thousand patented in Europe. He must moreover possess a familiar knowledge of the statute and common law on the subject, and the judicial decisions both in England and our own country, in patent cases. This service is important, as it is often difficult and laborious. Here is the first check upon attempts to palm off old inventions for new, or to interfere with the rights of others previously acquired. This is also the source whence the honest and meritorious inventor may look for aid and direction in so framing his specification as that he may be able to sustain his patent when issued, and find security and protection against expensive and fruitless litigation.

"Suitable qualifications for these duties are rare, and cannot be obtained without such compensation as they readily command in other employment. It will, undoubtedly, be wise in the government to affix such salary to this office as will secure the best talent and qualifications. Although an appeal is allowed by law, yet if a high character is given to it, this will be the best, as it is the most appropriate, tribunal, for judging of these subjects, and its decisions commanding respect and confidence, there will be but little inclination to take exceptions to its judgment. Thus will be cut off a fruitful source of law-suits, and our court-calendars[*] will cease to be crowded with cases arising out of the interfering rights of patentees. Meritorious inventors will be secure in their rights, and the public relieved from imposition and embarrassment. These are among the first of the objects and merits of the act of last session."

Public Loss by the Destruction of the United States Patent Office Building, and Measures proposed for its Restoration.

"In examining the subject referred to them, the committee[†] have been deeply impressed with the loss the country has sustained in the destruction, by the fire of the 15th December, of the records, originals, drawings, models, &c., belonging to the Patent Office. They not only embraced the whole history of American invention for nearly half a century, but were the muniments of property of vast amount, secured by law to a great number of individuals both citizens and foreigners, the protection and security of which must now become seriously difficult and precarious.

"Everything belonging to the office was destroyed—nothing was saved. There were one hundred and sixty-eight large folio volumes of records, and twenty-six large portfolios, containing nine thousand drawings, many of which were beautifully executed, and very valuable; there were also all the original descriptions and specifications of inventions, in all about ten thousand, besides caveats, and many other documents and papers.

"There were also two hundred and thirty volumes of books belonging to the Patent Office library, the cost of which was 1,000[‡] dollars. Some of these were procured prior to the passage of the act of July 4, 1836, making an appropriation of 1,500[§] dollars for purchasing a library of scientific works. Others were procured subsequently, for which 320[‖] dollars of that appropriation was expended.

"The model-cases, press and seals, desks, book-cases, and other furniture and effects belonging to the office, were estimated at 6600[¶] dollars.

"The Patent Office contained also the largest and most interesting collection of models in the world. It was an object of just pride to every American able to appreciate its value as an item in the estimate of national character, or the advantages and benefits derivable from high improvement in the useful arts—a pride which must now stand rebuked by the improvidence which exposed so many memorials and evidences of the superiority of American genius to the destruction which has overtaken them.

"The number of models was about seven thousand. Many of them displayed great talent, ingenuity, and mechanical science. The American inventions pertaining to the spinning of cotton and wool, and the manufacture of fabrics, in many respects exceed those of any other nation, and reduced so much the expense of manufacture, that the British manufacturers were reluctantly obliged, at the expense of a little national pride, to lay aside their own machinery, and adopt our improvements, to prevent our underselling them even in their home market. In this department were the inventions of Browne, Thorpe, Danforth, Couilliard, Calvert, and some others. The beautiful operative model of Wilkinson's machine for manufacturing weavers' reeds by one operation, was considered one of the most ingenious mechanical combinations ever invented. Of this character was Whittemore's celebrated machine for making wool-cards. There were several models of valuable improvements in shearing and napping cloth, patented to Swift, Stowell, Dewey, Parsons, Daniels, and others.

"In another department were several models of machines for manufacturing cut and wrought nails. The machinery for this purpose, which has reduced so much the price of that important article, was of purely American origin, and was invented by Briggs, Perkins, Reed, Odiorne, and several others.

"The models of improvements in

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[*] We little thought of hearing of a "Court-calendar" in the United States. Strictly, it is a correct application of the word, but is it not also *a straw which shows which way the wind blows?*

[†] Appointed by the senate of the United States.

[‡] £225.

[§] £337 10s.

[‖] £72.

[¶] £1485.

grist-mills, saw-mills, water-wheels, &c., were numerous.

"The application of steam-power to the driving of all kinds of machinery for propelling boats, locomotives, mills, and factories, has brought out a great number of American inventions and improvements, displaying a degree of talent, ingenuity, and science, highly creditable to our country. Some of the models in this department were very valuable. America claims the honour (contested, indeed, by England) of the first successful attempt to apply the power of steam to the propelling of vessels. The name of Fulton is associated with one of the noblest efforts of genius and science. It has often been regretted that no model was preserved of his steamboat, which was the first to demonstrate the practicability of making steam subservient to the purposes of useful navigation. There was, however, deposited in the Patent Office a volume of drawings elegantly executed by his own hand, delineating the various parts of the machinery he employed, and embracing three beautiful representations of his steamer making its first triumphant struggle against the opposing current of the Hudson. The steamer was represented passing through the Highlands, and at two or three other interesting points on the river, with a beautiful sketching of the surrounding scenery smiling as it were at the victory which science and art had at last achieved over the power of the winds and the waters, and at the opening era of steam-navigation, the benefits of which have since been so widely diffused. It contained also an account of his experiments on the resistance of fluids, and various estimates of the power required to propel vessels of various tonnage and form through the water at a greater or less speed. This volume, which should have been preserved among our choicest archives, shared the fate of everything else in the office. What sum would be too great to be expended in replacing it?

"The department of agriculture contained a great number of models of highly useful improvements in the implements of husbandry. The number of inventions which had for their object the advancement of the agricultural interests, was about fifteen hundred; those which pertained to navigation, were little short of a thousand. The inventions and improvements in factory machinery, and in the various manufactures, were upwards of two thousand. In the common mechanical trades there were as many more. It were vain to attempt to enumerate or classify them within the reasonable space of a report of committee. There was no art or pursuit to which ingenuity and invention had not lent their aid.

"That this great national repository should have received so little consideration heretofore as to be left so long exposed to conflagration, which has at last swept every vestige of it from existence, cannot be too deeply deplored."

After a few sensible and enlightened remarks upon the extensive benefit of such an establishment to the public at large, the committee proceeds:—

"The committee, therefore, believe that it is important to the interests of the country, as well as to the security of individual rights, that measures be immediately adopted to replace, as far as practicable, the records, drawings, and models, which have been destroyed. After much inquiry and consideration, the committee are satisfied that, notwithstanding the apprehensions and anxiety so generally entertained, a restoration is practicable to a very gratifying extent. The first step must be to procure, for the purpose of being copied and recorded anew, the original patents. In most instances, descriptions and specifications of the inventions, and in perhaps a sixth or eighth part of the cases, drawings also, have been annexed to the patents when granted. Drawings have been attached only when referred to in the specifications. The whole number of patents is a little upwards of ten thousand. It is believed that from six to seven thousand may be obtained for record. Many of the deficient drawings may be obtained from patentees, or may be supplied by the assistance of those whose familiar knowledge of the inventions will enable them, aided by the specifications, to delineate them with much accuracy. Many copies heretofore certified from the record to be used as evidence in the courts, will supply others.

"Of the models, such as were trifling and unimportant, containing no new principle or combination of mechanism, and not useful for any of the purposes before alluded to, it will not be necessary to replace. The whole number of models was about seven thousand. It is the opinion of the commissioner, and others most conversant with the subject, that three thousand of the most important can be replaced, which will form a very interesting and valuable collection, less numerous, indeed, but more select, and scarcely less useful, than that which has been destroyed. Some of these would be replaced by voluntary contribution. But the greater portion of them, even of those whose restoration would be most desirable, the committee are satisfied, can only be had by means in the hands of the government. If it were in the power of the government to compel the patentees to replace the models and drawings lost by its improvidence, it would be an onerous and unjust tax upon those who, by their ingenuity, and at their own expense, built up an institution which, in its connexion with manufactures, with agriculture, and even commerce itself, has done much to advance the prosperity of the country. They have paid into the Treasury 156,908 dollars* more than has been required to meet the expenses of the office, including the salaries of the officers employed in it; and the committee cannot hesitate in recommending the appropriation of that balance, to carry into effect the provisions of the bill which is herewith submitted.

"The measures to be adopted in selecting and obtaining the models and many of the drawings, are matters of detail, involving such a variety of circumstances and considerations, that it is impossible to make provision for them by law. That properly belongs, and should be intrusted, to a temporary board of commissioners. The sum required to replace the three thousand models, which would include all whose preservation would be most desirable, is estimated by the commissioners at 100,000 dollars†. The expense of transcribing and recording descriptions, specifications, drawings, and assignments, is estimated at 53,000 dollars‡. A judicious and economical expenditure of these sums, it is believed, will restore the records and models to the full extent contemplated by the provisions of the bill submitted. By the statement from the Treasury Department, it appears that the balance of the patent-fund on the 31st of December last, was 156,908 dollars, including moneys received for patents and copies prior to the act of July, 1836, which, though not expressly embraced by the terms of that act, properly belong to that fund. This balance will cover the expenditures above proposed of 153,000 dollars, together with 3100 dollars§ for record-books, desks, and other office-furniture, as per estimate, and leave a balance of 808 dollars‖.

"With such a restoration, and the addition of the specimens of fabrics and manufactures of various kinds, which are in preparation in a number of the manufactories and workshops of the country, to be deposited in rooms in the new building, pursuant to the act of July last, we shall soon have less reason than is now apprehended throughout the country to deplore the destruction of this great national repository. In two or three years the number of models will be scarcely less, and their character and value in the aggregate greatly improved."

The Committee conclude by stating that—

"Under existing circumstances, without written, pictorial, or model-record of any kind, it is apparent that the business of the office must either stand still, or proceed under very great embarrassment, which can be relieved only by the early action of Congress on the subject."

A bill is annexed for carrying these recommendations into effect; it contains additions (which we shall notice under another head) to the Act passed last year for the Reformation of the American Patents'-Law, and is therefore entitled, *A Bill in addition to the Act to promote the progress of Science and Useful Arts.*

It provides, for the especial purpose of furthering the replacement of the documents, models, &c., that—

Persons,—patentees before 15th of December last,—may present duplicate copies, &c., of the original records; and that the Commissioner of Patents shall make and keep copies of them without charge. Commissioner to take measures to obtain such copies, &c.

Such copies, certified by Commissioner, to be evidence without proof of the loss of originals. No patent granted prior to the 15th of December last, to be received in evidence, after the 1st of June next, unless it has been recorded anew: all assignments of such patents to be recorded anew.

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* £35,304. † £22,500. ‡ £11,925. § £698. ‖ £182.

Commissioner may grant new patent in lieu of one destroyed, bearing date of original; applicant for such patent to deposit satisfactory specifications, &c. Such patent shall be evidence; and shall protect the rights of patentees and assigns same as original.

Commissioner to obtain such duplicates of the more valuable and interesting models destroyed, as may be important to the public, assist Patent-Office, and protect public and patent rights. *Provided* that they be obtained at a reasonable expense, *and provided also* that the whole amount of expenditure do not exceed one hundred thousand dollars*. Temporary Commissioners to be appointed for above purpose.

Beneficial Effects of the United States New Patents' Act.

THE Act for the Reformation of the Laws in the United States, relating to Patents, entitled *An Act to promote the Progress of Science and the Useful Arts*, came into operation on the 4th of July last. Its effects are striking and instructive. The official examination now necessary, previous to the grant of a patent, has led to the following discovery, recently reported to the senate.

"It appears that about one-third of all the specifications are found, on examination, to contain no new principle, and that three-fourths of the residue are either too broad in their claims of originality, or are otherwise irregular or defective, and are required to be set right at the office, or to be sent back by the Commissioner for correction."

In the four months preceding the passing of the act, the number of patents issued from the patent-office amounted to	458
In the four months which followed the above epoch, there were only	70

A small part of this great reduction may probably be accounted for by the delays necessarily accompanying the first working of a new system, but that the lightness of the crop is principally owing to the early and judicious destruction of the worthless and noisome weeds which formerly shot over the Union from this hot-bed of speculation. We have, in addition to the above, the evidence of Dr. Jones, the Editor of the *Journal of the Franklin Institute*, a gentleman probably as conversant with the statistics of American Patents as any of his fellow-citizens. He says in the December number of that periodical,—

"Applications for patents now undergo a strict examination in the office before they are granted, and upwards of three-fourths of those examined have, under the provision of the existing law, been deemed imperfect and returned for amendment, or rejected for want of novelty."

No argument can be wanting to render evident the invaluable protection this previous examination throws before the public, and the increased credit and reputation it confers on patentees.

Intended Additions to the United States New Patents Act.

"FINAL Measures," we should think will scarcely be heard of in the legislation of the United States during this century. An opportunity of bettering the Patent Act, passed only in July last, has lately occured, and, as they say "down cast," *the occasion has been improved.*

A bill, whose title may be found at page 396, is now before the senate, in which, among others, clauses for the following objects stand for enactment:—

Patents may be issued to Assignee of the inventor.

Applicant to furnish drawings in duplicate; one set to be deposited in Patent Office, and the second to be annexed to patent, and considered part of the specification.

Committee may appoint agents in twenty principal towns for receiving and forwarding to Patent Office models, specimens, &c., intended to be subjects of patents, &c. Carriage of same to be paid for by the Patent Office.

Two examining clerks instead of one, and an additional copying clerk, to be appointed. Committee empowered to employ such temporary clerks as may be necessary.

On application of a foreigner-applicant being rejected for want of novelty, such applicant *to receive back from Treasury two-thirds of the money he may have paid.*

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* £22,500.

All moneys paid for patents, copies, &c., to constitute a fund for the maintainance of the Patent Office. Committee to lay annually, in January, before Congress, a statement of expenditure, &c.

Should this bill become law, the Americans will have made one step in advance towards the amelioration of international patent law, with regard, at least, to this country. Up to July last, they were the only nation who refused directly and indirectly a patent-right to a foreigner. At that period they placed foreigners on the same footing in America, as their own citizens possessed in countries foreign to them, and "subjects of the king of Great Britain" had the enviable pre-eminence of paying about *twenty times as much* as a native, and about twice as much as any other foreigner, because we make Americans pay a similar sum for a patent in England. The money too, at Washington, as at London, once paid, was irrecoverably gone. By the American new bill, two-thirds of the 112*l.* are to be returned if the application should be refused. This is imitating the courtesy of France in the same case. What will England now do? Supposing she continue to squeeze and plunder her own ingenious population, driving a singularly meritorious class frequently into insolvency, or, what is worse, into the net of the blood-sucking capitalist; can she now so treat the subjects of other governments, who, without insisting upon reciprocity as formerly, refuse to make extortion legal, and who do unto Englishmen what England refuses to do unto their countrymen? Surely justice, honour, policy, courtesy, will one day or other prevail against the dealers in wax, and parchment, and tin-boxes, and ———!

Hopes deferred. Second Series.

A JOURNAL of the Patents for Inventions Bill. Session 1837!

14th Feb.

Patents for Inventions.—Bill to alter and amend the Patent Laws, and for better securing to individuals the benefit of their inventions, *ordered* to be brought in by Mr. Mackinnon and Mr. Baines.

15th Feb.

Patents for Inventions' Bill, "to amend the practice relating to Letters-Patent for inventions, and for the better encouragement of arts and manufactures," presented and read 1°; to be read 2° on *Wednesday*, 1st of March, and to be *printed*.

1st March.

Patents for Inventions,—Second Reading *deferred* till *Wednesday* 12th of April.

12th April.

Patents for Inventions' Bill—Second Reading *deferred* till *Wednesday* 17th of May.

In the "Votes and Proceedings of the House of Commons, *Mercurii*, 17° *die Maii*, 1837," the *Wednesday* to which it had been last *deferred*, there is no notice of this unfortunate Bill. It is true the House was counted out at eight o'clock on that day, there not being forty members present, but the Bill does not appear in the list entitled "Dropped Orders of the Day of Wednesday, 17th May, 1837."

We cannot, therefore, report *progress*, even at the imperceptible rate of progression which has hitherto attended the Bill[*].

Patent-Law Grievance. No. XV.

THE inventors of this country, and the introducers of inventions of other countries into this, were obliged to pay down to the attorney-general and other agents, &c., of the Government during the ten years ending December, 1834, more than £313,000, and during the past year above £42,000 (being at the rate of £420,000 in ten years). It is to be observed that these enormous extortions are *exclusive* of the drawings, engrossings, and all the other charges of the patent-solicitor.

The penalties inflicted on the inventive genius of Britain during the *present* year, up to the 25th ult., in the shape of government stamps and fees on patents, amount to more than £20,000!

N.B. This sum has been paid in *ready money* on taking the first steps, and as many of the inventors are poor men, (*Operatives*,) and a great many others of them persons to whom it would be very inconvenient to pay at least £100 down, they have been obliged to go into debt, or mortgage or dispose of their inventions, either wholly or in part, &c.

[*] And in the printed Votes to the 25th of May inclusive, "hope never comes!"

NEW PATENTS. 1837.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second that on or before which the Specification must be delivered and enrolled.——The abbreviation *For. Comm.*, signifies that the invention &c., is "a communication from a foreigner residing abroad."

April *contd.*

86. Henry William Craufurd, John-st., Berkeley-sq., *Middx.*, R. N.; for an improvement in coating or covering iron and copper for the prevention of oxidation. April 29.—Oct. 29. *For. Comm.*

87. Alexander Dixon and James Dixon, Cleckheaton, near Leeds, *York.*, Manufacturing Chemists; for improvements in dyeing, by the application of materials not hitherto so used. April 29.—Oct. 29.

88. Joseph Barker, Regent's-st., Lambeth, *Surry*, Artist; for an improvement in the construction or making up of umbrellas and parasols. April 29.—Oct. 29.

Total, April...24.

May.

89. Jean Baptiste Mollerat, Leicester-sq., *Middx.*, Manufacturing Chemist; for improvements in the manufacture of gas for illumination. May 2.—Nov. 2.

90. John Heathcote, Tiverton, *Devon.*, Lace Manufacturer; for improved methods of manufacturing ornamented work or figures upon or applicable to gauze muslin and net, and divers kinds of cloth, stuff, or woven textures, and also apparatus to be used in ornamented work. May 4.—Nov. 4.

91. Thomas Wells Ingram, Birmingham, *Warw.*, Horn Button Manufacturer; for improvements in the manufacture of certain descriptions of buttons, and in the tools used to manufacture the same. May 4.—Nov. 4. *For. Comm.*

92. Thomas Baylis, Tamworth, *Staff.*, Civil Engineer; for improvements in heating and evaporating fluids. May 6.—Nov. 6. *For. Comm.*

93. Henry Ross, Leicester, Worsted Manufacturer; for improvements applicable to the combing of wool and goat hair. May 6.—Nov. 6.

94. George Hayman, Saint Sidwell-st., *Exeter*, Coach Builder; for improvements in two-wheel carriages. May 6.—Nov. 6.

95. Angus Robertson, Peterborough-court, Fleet-st., *Lond.*, Gent.; for machinery for sculpturing and working marble, stone, alabaster, and other substances, and for taking copies of the works produced thereby, or of similar works produced by the ordinary means, and also an improved process of taking casts of the human figure or other forms. May 6.—Nov. 6. *For. Comm.*

96. Thomas Bell, South Shields, *Durham*, Manufacturing Chemist; for improvements in the manufacture of sulphate of soda, which improvements are applicable to other purposes. May 8.—Nov. 8.

97. William Nairne, Flax Spinner, Millhaugh, near Methven, *Perth.*; for improvements in the machinery of reels used in reeling yarns. May 8.—Nov. 8. *For. Comm.*

98. Peter Steinkeller, Wenlock-rd., *Middx.*, Gent.; for plates or tiles made of zinc or other proper metal, or mixtures of metals, applicable to roofs or other parts of buildings. May 8.—July 8. *For. Comm.*

99. John Spurgin, Guildford-st., *Middx.*, M.D.; for improvements in the mode of propelling vessels through water, and part of which means may be applied to other useful purposes. May 8.—Nov. 8.

100. John Hague, Castle-st., Wellclose-sq., *Middx.*, Engineer; for improvements on wheels for carriages. May 10.—Nov. 10.

101. James Boydell, Jun., Hawarden, *Flint.*, Esq.; for improvements in propelling carriages. May 11.—Nov. 11.

102. William Bell, Edinburgh, *Scotland*, Esq.; for improvements in heating and evaporating fluids. May 11.—Nov. 11.

103. Edward Austin, Warwick-place, Bedford-row, *Middx.*; for improvements in raising sunken vessels and other bodies. May 12.—Nov. 12.

104. Pierre Barthelemy Guinebert Debac, Brixton, *Surry*, Civil Engineer; for improvements applicable to railroads. May 13.—Nov. 13.

105. William Rhodes, Gent., and Robert Hemingway, Mechanic, both of Earl's Heaton, *York.*; for improvements applicable to machinery for carding and piercing wool, in process of manufacture in woollen mills. May. 22.—Nov. 22.

106. George Nelson, Leamington Priors, *Warw.*, Gent., for processes by the use of which isinglass may be improved. May 22.—Nov. 22.

107. Samuel and William Smith, Luddenden Foot, near Halifax, *York.*, Worsted Spinners; for improvements in machinery for combing or clearing sheep's wool and goat's hair. May 23.—Nov. 23.

108. Elijah Leak, Hanley, *Staff.*, Engineer and Lathe Maker; for improvements in the construction of shutters and sashes for windows of buildings, which improvements are also applicable to hot-houses, or conservatories, carriages, and other purposes, and in the mode of fitting or using the same. May 23.—Nov. 23.

109. Charles Pierre Devaux, Fenchurch-st., *Lond.*, Merchant; for apparatus for preventing the explosion of boilers or generators of steam. May 23.—Nov. 23. *For. Comm.*

110. Baron Henry de Bode, Maj. Gen., Edgeware-rd., *Middx.*; for improvements in apparatus for retarding and stopping chain or other cables, or ropes, on board ships. May 23.—Nov. 23.

111. Charles Joseph Freeman, Frederick's-place, Kennington-lane, *Surry*, Gent.; for improvements in the apparatus called rolls, for rolling iron or other metals, applicable to rails for roads, and bars for other purposes. May 25,—Nov. 25.

METEOROLOGICAL JOURNAL FOR APRIL, 1837; KEPT AT BLACKHEATH ROAD.

Day of Month		Barom. 9 A.M.	Ther attch.	Barom. 3 P.M.	Ther attch.	Thermometer		Daily Temp.	Solar Var.	Rad.	Clouds.		Winds		Direction of Wind		Lunation.	WEATHER, &c.
						Min.	Max.				A.M.	P.M.	A.M.	P.M.	A.M.	P.M.		
Satur.	1	30·030	50°	29·971	52°	29°8	47°5	38°7	17°7	25°	5	5	2	2	WNW	NNW		Fine; *cumuli* and *cum.-stratus.* Clear frosty Night.
SUN.	2	30·012	49	29·970	50	26·9	46·0	36·4	19·1	24	5	8	1	1	NW	W		Sharp hoar frost, with ice; hazy.
Mon.	3	29·583	52	29·586	53	35·5	46·9	41·2	11·4	33	5	3	4	3	wbs	W		Windy; shower of rain and sleet at sunset; fine Night.
Tues.	4	29·732	50	29·750	52	30·2	43·8	37·0	13·6	28	7	10	1	1	WNW	NE		Cloudy throughout.
Wed.	5	29·775	47	29·794	49	33·9	43·8	38·9	9·9	33	10	5	2	3	NEbE	NE	●	Strong wind. *Cirro-stratus.* Frosty Night.
Thurs.	6	29·978	46	29·981	49	30·8	44·9	37·8	14·1	29	7	3	2	1	NbE	N. SE		*Cum.* and *cum.-stratus.* Overcast.
Friday,	7	30·158	46	30·200	49	28·3	42·4	35·4	14·1	25½	6	6	1·3	2	N	NNE		Frost, with ice, A.M. Squalls, with sleet & hail. Windy
Satur.	8	30·409	44	30·400	47	32·0	43·2	37·6	11·2	30	2	3	3·4	3	NbE	NE		Wind very high; *cumuli.* [& cloudy at Night; *aur. bo.*
SUN.	9	30·358	44	30·308	45	27·2	39·0	33·1	11·8	26	5	6	4	3·2	NE	NE		Thick ice; high wind at intervals. Snow at 6 P.M.
Mon.	10	30·179	42½	30·052	45	26·0	39·1	32·7	13·1	24	7	8	2	2	NNE	N		Frost very severe; snow-showers. { Ice ½ inch thick
Tues.	11	29·755	43	29·696	45	23·9	40·2	32·0	16·3	22	5	7	0	1	S	SE		Do., do., with rime. Cloudy. { in one night.
Wed.	12	29·740	46	29·760	45	23·2	40·5	31·9	17·3	22	5	7	1	1	E	E	☽	Do., do. Cloudy, with light snow.
Thurs.	13	29·932	43	29·949	45	33·9	39·0	36·5	5·1	32½	10	10	2	2	NE	NE		Cloudy and cold throughout.
Friday,	14	29·980	45	29·915	47	32·8	45·4	39·1	12·6	32½	7	3	0	0	N	N		Fair; more moderate. Frosty Evening.
Satur.	15	29·755	47	29·630	49	26·9	47·0	37·0	20·1	25	2	0	0	1		E		Frost, with ice; fine.
SUN.	16	29·481	49	29·500	48	33·4	37·6	35·6	4·2	31	10	10	1	2	NW	WNW		Snow early A.M. A dark day.
Mon.	17	29·750	48	29·802	48	34·0	45·8	39·9	11·8	33	10	6	3	2	WNW	NNW		Cold wind and cloudy. Fair P.M.
Tues.	18	29·955	48	30·000	49	35·8	42·9	39·4	7·1	35	10	10	1	1	Nbw	N		Cloudy.
Wed.	19	30·042	50	30·001	51	40·4	52·0	46·2	11·6	40	10	5	0	0		SE		Do. till 2 P.M. Evening fine.
Thurs.	20	29·901	54	29·900	55	32·5	55·6	44·1	23·1	29	7	9	1	2	W	WNW	○	Fine, A.M. Squall at 2 P.M., *during which Therm. fell*
Friday,	21	29·852	52	29·775	52	32·0	49·9	40·9	17·9	29	9	6	1	2	SW	sbw		Hoar frost; rain and snow. [10°.
Satur.	22	29·642	52	29·648	52	32·4	47·1	39·8	14·7	28½	7	8	0	1	NNW	NW		*Cumuli* into *cum.-stratus*, with light showers of hail.
SUN.	23	29·750	52	29·748	52	37·3	48·5	42·9	11·2	37	9	9	2	2	WSW	WSW		Rain early A.M. Squally. Cloudy Night.
Mon.	24	29·802	53	29·840	56	36·1	56·1	46·1	20·0	33	3	7	1	0	SSW	W.		Fine. A very heavy shower of hail and rain P.M.
Tues.	25	30·000	55	30·002	57	32·2	57·0	44·6	24·8	30	7	7	0	2	W	SSW		Do. *Cirro-cumuli.*
Wed.	26	29·932	57	29·941	59	44·0	58·6	51·3	14·6	44	4	2	1	1	W	W		Do. *Cumuli.* *Weather getting milder.*
Thurs.	27	29·875	56	29·816	57	38·4	55·0	46·7	16·6	35	7	7	2	2	SSW	SW. NW	☾	Much cloud. Light rain. *Aurora* at Night.
Friday,	28	29·781	57	29·700	58	36·5	56·5	46·5	20·0	32	8	5	1	1	S	S		Do. Do. Cloudy Night.
Satur.	29	29·601	57	29·450	57	44·0	53·0	48·5	9·0	42	10	10	2	2	sbE	SSW		Cloudy, with rain.
SUN.	30	29·550	58	29·556	60	48·0	59·5	53·7	11·5	46	10	4	3	3	SW	SW		Do. Windy. *Cirrus* and *cirro-stratus.*
Mean		29·876	49	29·855	51	33·28	47·46	40·38	14·18									

Bar. Max. 30·409 in. on the 8th.
Bar. Min. 29·415 in. „ 29th.

Ther. Max. 59·5° on the 30th.
Ther. Min. 23·2° „ 12th.

Lowest point of Rad. 22°, on the 11th and 12th.
Rain fallen (including snow), 1·525 in.

THE BAROMETER.

INTRODUCTION.

So much does the habitual use of any instrument, or piece of apparatus, (whether intended for experimental inquiry, or for the more ordinary purposes of life,) engender a forgetfulness of the principles on which it is constructed, or on which it acts, that we are scarcely aware of the extensive and delightful field of inquiry into which we should be led, on endeavouring to trace, and account for, the mode of operation, and the physical laws which determine the utility, of the mechanical contrivances which the ingenuity of man has invented for his own benefit and convenience: and yet such inquiries are, perhaps, among the best modes of attaining a knowledge of the principles of science; for, so linked are the sciences one to another,—so imperceptibly are the boundaries which separate them shaded off, that a thorough knowledge of the principle of construction and of operation of any one instrument, would lead us into more sciences than, at first, we should be apt to suppose.

There are, it is most certain, very many instruments, which a moderately-intelligent member of society is in the habit of encountering and applying to his use, each of which, if properly studied in all its bearings, would give a vast fund of valuable information, independently of the mere *use* to which the instrument itself is applied. Among such instruments is the *Barometer*, a piece of apparatus, whose action depends upon principles as beautiful, perhaps, as any in the whole range of philosophical inquiry. We propose, therefore, to give a popular account of this instrument; its history, mode of action, and the atmospheric phenomena on which that action depends.

The first step in such an inquiry is, a consideration of the atmosphere which surrounds us,—that invisible garment, whose presence is necessary to the very existence of animal and vegetable life, and to the conduct of this globe, under the physical laws by which it is governed.

I.—THE ATMOSPHERE.

THE atmosphere belongs to that class of fluids which, for distinction's sake, is called elastic: this distinction is not strictly correct, for *all* fluids are elastic (that is, they can be compressed into a smaller space, and regain their former bulk when the pressure is removed); but the range of compressibility in liquids is so very limited, that, for all practical purposes, it is sufficiently correct to designate them as non-elastic.

The atmosphere, then, is a compound elastic fluid, invisible, and inodorous, of which the constant component parts are oxygen and nitrogen. Carbonic acid gas, and aqueous vapour, together with other bodies, are constantly present, but, being due to irregular sources, they are not considered as component parts of the atmosphere.

According to the best experiments, the ordinary constituents of the atmosphere appear to be in the following proportions:—

	By measure.	By weight.
Nitrogen	77·5	75·55
Oxygen	21·	23·32
Aqueous vapour	1·42	1·03
Carbonic acid	0·08	0·10
	100.	100.

or, speaking only of the two more important elements,—

Oxygen	21	23
Nitrogen	79	77
	100	100

This invisible compound fluid possesses many of the properties of solid matter, but, at the same time, possesses many which are peculiar to gaseous bodies. In common with solid matter, it possesses the properties of compressibility, inertia, mobility, and weight: these we may term the *mechanical* properties of air, and of these we will treat before we consider those properties which belong only to fluids.

Impenetrability implies an attribute by virtue of which no two bodies can at the same time occupy the same place, and is one of the chief properties which give rise to our notion of the word "matter." Now, that no two solids can, at the same time, occupy the same place, seems a truism, the bare mention of which is childish, because our senses have always told us the same thing; but when we come to consider an invisible fluid like air, the same evidence fails us: we cannot *see* that it is true, and the proof of its truth becomes more desirable in proportion as it is more difficult. Now, that air, invisible though it be, possesses this property, is easily shown thus: plunge an inverted goblet into a body of water, observing, however, that every part of the edge touch the surface of the water at the same moment; it will be found that, whatever be the depth of the water in the vessel, the goblet will not get filled, for there will be a portion unoccupied by water, varying in height with the depth to which the goblet is plunged. Now, this portion is occupied by the air which originally filled the goblet, and the circumstance of that air occupying less than its original bulk, is no proof against its impenetrability, but only shows that it is compressible, or that its atoms are readily brought nearer together; but no pressure of the water will enable it quite to fill the goblet; thus showing, that however the air may be compressed, it is never annihilated. If the goblet be introduced obliquely into the water, the air will have time to escape before it is imprisoned between the glass and the water. Again, if a diving-bell is plunged to any depth into the sea, it never becomes quite filled with water: at 34 feet below the surface, it is exactly half filled, from causes to be afterwards explained.

Inertia and *mobility* are those properties by virtue of which a body will not move until it is forced to do so by some active power, and that, when in motion, it will not stop without some equal preventive power. Now, that air possesses these properties, is at once evident from the phenomena of *wind*, which is nothing more than moving air. When we stand still in a calm day, we feel no wind, but if we run, the face feels as if a wind were blowing upon it. Now this arises from the first of these two properties; the air will not move, unless some force impress it: our bodies then become the moving force, and the degree of power necessary to move the air is measured by the degree of wind which we feel in our faces; it is, in fact, an index of the reluctance of the air to begin to move: but when air is once in motion, as in the case of ordinary wind, we find as much force is necessary to stop it as was required to put it in motion; and the degree of force with which the wind rushes against any stationary object, measures the power necessary to stop its motion: thus, when a hurricane, such as frequently occur in the West Indies, levels buildings to the ground, and tears up trees by the roots, we are convinced that the air is moving with a velocity which requires, for its complete suppression, a power greater than that by which a tree clings to the earth, or a building to its foundation.

To these properties we will now adduce *weight*. That air possesses weight is, to a novice in philosophy, one of its greatest wonders. It has been accurately determined, by weighing a vessel containing a given quantity of pure air, *i. e.*, air deprived of its carbonic acid and aqueous vapour, and then, after extracting the air, weighing the vessel again, that 100 cubic inches of atmospheric air, when the thermometer indicates 60°, and the barometer 30 inches, weigh rather more than 31 grains, which is about $\frac{1}{815}$th part the weight of water. Although the delicacy of this proof places it beyond the range of familiar experiment, yet, that air *has* weight, is abundantly shown in other ways. We know that the silk, car, &c., which form a balloon, possess *weight*, and yet it ascends in air; this shows that the balloon, with the carburetted hydrogen contained in it, is not so heavy as an equal bulk of atmospheric air. The same remark will apply to clouds, which we know are composed of water, and, therefore, to possess weight; yet they are upheld at a considerable elevation, merely because the air beneath them is, bulk for bulk, heavier than the clouds.

Now, from these four properties, viz., *impenetrability*, *inertia*, *mobility*, and *weight*, we might trace a great range of atmospheric phenomena; but the influence which two other agents,—*heat*, and *aqueous vapour*,—exert on air, is so extensive, that it is impossible to arrive at just notions on the subject, without first considering those agencies.

Whatever may be the nature of heat, whether material or not, we know that one of its most important effects is the expansion of any body into which it enters, and thus, by removing the component particles of a body farther from each other, rendering any given bulk of the body lighter than it was before the application of heat. In solids, this expansion is very limited, and requires very nice investigation to measure it. In non-elastic fluids, such as water, mercury, alcohol, &c., the expansibility is much greater ,but it is at the greatest in elastic fluids, such as air; for the attraction of cohesion being entirely non-existent among its particles, any foreign agent tending to remove the particles farther asunder, is aided in that object instead of retarded. In illustration of this we may state, that if a given bulk of *steel*, at 32° of temperature, be heated to 212°, its bulk is increased by about $\frac{1}{350}$th part of the whole; and glass nearly in the same degree. In liquids, the ratio is so much increased, that, with the same increment of temperature, mercury expands about $\frac{1}{63}$rd, water about $\frac{1}{25}$th, and alcohol about $\frac{1}{9}$th; those being the most expansible which boil at the lowest temperature. But in air, the expansion amounts to more than one-third of the whole bulk. This, however, is not the most important point of distinction between the relative expansibilities of bodies in different states: it is found that solids and liquids expand in a greater ratio with a given increment of temperature when they are near the boiling point, than at a lower temperature; thus steel, which expands $\frac{1}{350}$th, on being raised from 32° to 212°, or 180°, if we take half the quantity, or 90°, we shall find that the expansion is *less* than $\frac{1}{700}$th from 32° to 122°, and more than $\frac{1}{700}$th from 122° to 212°. The same applies likewise to liquids, but in a less degree to mercury than any other liquid; hence its value for thermometers, on account of its nearly equable expansion.

But in gaseous fluids, the expansion is rigorously equal for every degreeof temperature; and it is a most important law, that all gases expand, not only equably with equal increments of temperature, but likewise equal to each other; that is, $\frac{1}{480}$th of the whole bulk for each degree of temperature, or 0·375 from 32° to 212°.

It is at once, therefore, obvious, that a most material change will take place in the equilibrium of an aerial mass, if one portion be, from any cause, more heated than that which surrounds it; because, as the particles of which it is composed are subject to the same law of gravitation which regulates solid bodies, those which are most expanded rise to make way for those which, by a closer aggregation of particles, possess greater specific gravity. We shall hereafter see the immense importance of this adjustment of aerial particles.

The influence of heat, in modifying the state of elastic fluids, being thus shortly alluded to, we will now consider a distinction between elastic fluids, which leads to many important results. Oxygen and nitrogen, whether considered separately, or in the combined state which forms our atmosphere, are *permanently-elastic* fluids, which means, that neither compression nor abstraction of heat has ever converted them into the liquid state; but with aqueous vapour it is different (as it is likewise with some of the gases). Aqueous vapour, or steam, is not permanently elastic, as, unless its temperature be maintained to a certain height, depending on its density, it resumes the liquid state in which it first existed. The same remark applies to mercurial and alcoholic vapours; whether the gases, such as oxygen or nitrogen, might be liquefied by excessive abstraction of heat, accompanied by enormous pressure, is an unascertained point; it is sufficient to state, however, for our present purpose, that no means hitherto tried have succeeded in that object.

The formation of steam from water goes on slowly, at almost every temperature, but it is at 212° that boiling takes place (that is, under the influence of the atmosphere; for if we remove that influence, by placing water in vacuo, it boils at about 72°); the elastic force of the vapour thus formed is, however, proportional to the temperature of the water from which it ascends, and at a low temperature the steam struggles slowly and silently against the pressure of the superincumbent atmosphere.

Until within a very few years, it had been always supposed that the pressure which interferes with the rapidity of evaporation, is excited by the atmosphere *as a whole;* but the investigations of Dalton and Wollaston have rendered it probable, that to the aqueous vapour contained in the atmosphere is alone due all that pressure which retards evaporation. It is believed that the component parts of the atmosphere are not chemically combined, but merely mechanically mixed; that there are, in fact, four distinct and separate atmospheres, of oxygen, nitrogen, carbonic acid gas, and steam: that these atmospheres exist independently of each other, and exert pressure, independently, on the earth's surface (those pressures being believed to be in the ratio of the volumes of the different gases); and that they reach to different heights therefrom; the nitrogen atmosphere being 54 miles, steam 50 miles, oxygen 38 miles, and carbonic acid 10 miles. But the point to which we more particularly allude, is the recently-formed opinion, that the preventive pressure, by virtue of which evaporation and ebullition are retarded, is excited solely by the *steam atmosphere;* that one sort of gaseous fluid offers no obstruction to the passage of another sort through it; that the repulsion existing between the particles of a gas is exerted only against its own kind, and that it is that repulsive energy in the steam atmosphere which retards the formation and issue of more steam from the surface of the liquids; that if the air were *perfectly dry* (that is, free from the steam atmosphere,) evaporation would go on to as great extent, though more slowly, from a liquid surface, as if no air existed above it; and that there is a sort of a contest always going on between the steam already formed and existing in the atmosphere, and that which is about to leave the surface of bodies of water, the repulsive pressure of the one being opposed to the elastic tension of the other; and as an increase of temperature in the water increases the tension of the steam generated therein, the rapidity of evaporation varies with every change of temperature. The manner in which a balance is maintained between the downward pressure of the aqueous vapour in the atmosphere, and the elastic tension of that which is forming in water, under the influence of heat, will be better understood by reference to the following table.

At the temperature of 32°, evaporation produces an atmosphere of steam above the surface of water, of which the tension is equal to one-fifth of an inch of mercury; that is, it presses on surrounding bodies with a force equal to the weight of one-fifth of a vertical inch of mercury of the same horizontal surface. The tension, or pressing force, increases with increase of temperature as follows:—

At 50°	0·373	At 212°	29·992
68	0·682	233	44·883
86	1·206	251	59·884
104	2·087	264	74·805
140	5·694	275	89·766
158	9·019	285	104·727
176	13·855	302	134·649

This table will give an idea of the manner in which the giant power of the steam-engine is called into existence. Water heated in a vessel of limited dimensions generates a steam atmosphere above it: this atmosphere, having no room for expansion, gradually increases in density and elastic tension, until at 212° it presses on the inner surface of its containing vessel with a force equal to the atmospheric pressure on the outside; but above this temperature its elastic power becomes enormously increased, so that at 302° it presses with a force of more than 60 pounds on each square inch of surface, and at 343° with a force of 120 pounds.

When we consider that the ocean covers about three-fourths of the whole surface of the globe, and that steam rises from water at every temperature, but in different quantities according to the temperature, the vast importance of that element of the atmosphere will at once present itself to our view.

We shall now proceed to detail the atmospheric phenomena which are brought about by the mechanical properties belonging to the unvarying elements of the atmosphere,—oxygen and nitrogen,—and by the ever-changing proportion in which heat and aqueous vapour are mixed with it.

Air possessing weight, it necessarily follows that that portion of it which is nearest the surface of the earth has to bear the pressure of all which is above it, and becomes therefore more condensed; now, when we say that 100 cubic inches of air weigh 31 grains, we allude to air taken from that stratum which is nearest the earth: but it has been found, in conformity with what has been just stated, that its specific gravity decreases as it is further removed from the earth. This has been found true to all the heights which man has been able to reach, and calculations founded on the rate of expansion at accessible heights, has determined the limit of the atmosphere to be about fifty miles. Many have supposed this rarefaction to go on into infinite space, but the most distinguished philosophers agree in opinion, that beyond a certain distance, centrifugal force, which increases the further removed it is from the centre of the earth, combined with the extreme tenuity of the air, and the diminution of gravitating attraction, would overpower the repulsive tendency of its particles, and prevent them from receding farther from the earth. But although we cannot tell the weight of a given bulk of air at the highest stratum, yet we find that a column of air one inch square, and reaching from the earth to the summit of the atmosphere, weighs about 14½ pounds, and as we likewise know that a cubic inch at the earth's surface weighs somewhat less than one-third of a grain, we can calculate that, if the atmosphere was of *equal density* throughout, it would be between four and five miles high.

That a column of air, one inch square, and reaching to the top of the atmosphere, weighs 14½ pounds, is a fact that common *weighing* will not indicate, but we find it from other evidence; the reason why weighing will not decide it, is to be found in the remarkable property which distinguishes fluids from solids, which is this:—if we take a solid body, a mass of iron for instance, it presses only in one direction towards the centre of the earth, and exerts no lateral or upward pressure; but in fluids, whether liquid or aeriform, any impulse communicated to them spreads equally in every direction: this results from the perfect freedom with which the particles move among each other, and is shown in numerous familiar examples. An orifice being made in the side of a vessel containing water, the water rushes out with a force exactly equal to the pressure which the body of water above the orifice exerts on the water beneath that level; and in the same way, if the vessel be quite full, and closed at the top, through which a tube passes to the water within, if the tube be likewise filled with water, the upper stratum of water in the vessel presses *upwards* against the cover with the same power with which it presses on the water beneath it: and so likewise in aeriform fluids, if a bladder be filled with compressed air, and an orifice made in it, the air will escape with just as much velocity, if the hole be made in the top or the side, as if it were at the bottom.

It follows, therefore, that the pressure of 14½ pounds on the square inch which the atmosphere exerts at the earth's surface, is exerted equally in every direction upwards, downwards, and laterally, and this enormous pressure, acting on such an extremely attenuated body as air, forces it into the most minute apertures, and it is thus that we feel no inconvenience from the weight of such a superincumbent pressure, (which is not much less than fourteen tons on the whole body of an ordinary man,) but every part of the human system being vascular, and containing air, the pressure from without is counteracted by that from within; and were it possible for us to exist for a few minutes in a vacuum, the body would be burst by the efforts of the air within it to escape. But immersed as we are in the atmosphere, the body may be compared, as Professor Robison observed, to a sponge, which is not crushed by the weight of water upon it, however deep it may be plunged into the sea, simply because there is water within its cavities, as well as without. And this brings us to the reason why we cannot weigh the whole pressure of the atmosphere by ordinary means, as the scale of a balance, for instance, is pressed *upwards* by the air beneath it in exactly the same degree as it is pressed *down* by the air above it. But whenever we can free one side of any object from atmospheric pressure, we can then ascertain the amount of force pressing on the other side; thus, in a well-known experiment, two hollow hemispheres are simply placed edge to edge, but without any mode of fixing or securing. The air is then extracted from the central hollow, and it is found that the two halves cannot be separated by a less force than fourteen times as many pounds as there are square inches in the section of the sphere. The power of a fly to fix itself on the ceiling or wall of a room depends upon this upward and lateral pressure of the air; there is a muscular power by which it is enabled to form the bottom of the foot into a hollow shape between which and the ceiling a vacuum exists: he is then upheld by the pressure of the air against the opposite side of the foot. Again, in the operation of cupping, the air is removed from a small external part of the body, in consequence of which the internal air thrusts out the skin, and with it the blood-vessels near the part, into a swelling protuberance. Similar instances are numerous, but these will suffice.

Having thus shown the nature of the pressure which the atmosphere exerts on all bodies at the earth's surface, we will proceed to the consideration of the manner in which this pressure gave rise to the construction of the Barometer, and then, after a description of the mode of operation of that instrument, we will briefly detail the changes in the atmosphere which it is intended to indicate.

II.—The Pump and the Barometer.

For nearly 2000 years, the common pump has been in use for the same purposes to which it is at present applied, viz., the raising of water from depths which could not otherwise be conveniently reached; or, at least, whether it has been *used* so long or not, its adaptation to that purpose was known; but, for 1700 years afterwards, the principle on which its action depends was unknown. In the middle ages, the convenience of its application was sufficient to satisfy the minds of those who used it, without suggesting any inquiry into the causes of the phenomenon; indeed, the state of mind, among nearly all classes in those ages, was based upon such erroneous grounds, that an inquiry into the laws by which Nature so beautifully governs the universe, was looked upon as an unhallowed presumption: the mind was permitted to rust, for fear that its polish should reflect too much light. But, happily for mankind, the age of Galileo arrived, and with it a more just and ennobling tone of thinking, the fruits of which subsequent ages have amply gleaned.

The ancient philosophers were wont to explain natural phenomena by a series of dogmas of universal application, which, through the writings of Aristotle and others, were for many ages taught and persisted in, with the melancholy perseverance in error which but too often characterizes the human mind. Thus, the removal of air from a tube, by means of the mouth, or by a piston, and the consequent ascent of water in the tube, was explained by referring to Nature a great abhorrence of a vacant space. A tube placed one end in water, and the other end in the mouth, if, they say, the air be "sucked out," water will rise and fill the tube, because "Nature abhors a vacuum." Nobody thought of asking how Nature, which is nothing more than physical phenomena, taken as a whole, became thus personified, and transformed into a being susceptible of passions. But at this epoch, *doubt was not invented;* and it was only the occurrence of a real and positive inconvenience to man, that led the way to a knowledge of the simple cause, why water rises in an empty tube.

Certain Florentine engineers, having caused a well to be sunk, and a pump to be erected, it was found that the water would not ascend to the surface. All were astonished at this fact, and the cause was discussed with great eagerness. Upon inquiry into the circumstances, it appeared that the distance of the piston of the pump to the water in the well, was more than 18 palms, or 34 feet, and that this appeared to be the source of failure.

Galileo was consulted; and it is now a matter of doubt, whether his solution of the difficulty was given satirically or seriously. His reply was, "that Nature's abhorrence of a vacuum extended only to the height *of 32 feet.*" This peculiar predilection of Nature for a certain number of feet, was a capital idea for the mass of wonderers, as it furnished an easy solution of the difficulty; but Galileo, and a few of his distinguished pupils, Torricelli, Viviani, and Ricci, were not so satisfied. If, indeed, they were aware of the true principle, they now saw how it might be applied; but if, as is most probable, they were ignorant of it, then the manifest absurdity into which this dilemma had drawn them, would stimulate their vigorous minds to search for and discover the hidden principle: and, indeed, we have every reason to suppose that, from the circumstance above noticed, they turned their attention to the subject, and elicited results which finally gave birth to the barometer.

The great question to solve was, "Why does water rise in a pump?" A principle of *suction* had been advocated. The piston was said to suck the water out of the well; but Torricelli conceived that it might be the pressure of the external atmosphere which forced the water up into a barrel, from which the piston had previously extracted the air. We may suppose him to reason thus: that the vertical pressure of a column of water 33 feet high, equals that of the entire height of the atmosphere; and, as a necessary consequence of that hypothesis, if true, it will equal the pressure of a column of any other fluid, of a height in the inverse ratio of its specific gravity as compared with water; of a greater height if the fluid be lighter, and of a less height if it be heavier, than water; which ratio would assign to mercury a height of about 2½ feet.

To test this opinion, he took a glass tube, 3 feet long, and closed at one end; filled it quite full of mercury, and then inverted it into a basin of mercury, preventing the admission of air into the tube, by covering the open end with the finger: the finger was then removed, and a communication opened between the mercury in the tube and that in the basin, and it at once sank in the tube to the height of about 30 inches.

Here was a confirmation of his views. The mercury in the basin was pressed in one part by a column of mercury 3 feet high, and in every other part by the superincumbent atmosphere: the former proved to be, for a given horizontal surface, the greater pressure of the two, and the mercury fell to such a height as exactly equalled the atmospheric pressure. Now it can be easily determined that a column of water, an inch square and 33 feet high, and a column of mercury, an inch square and 30 inches high, each weigh between 14 and 15 pounds; and from this conjoined testimony Torricelli concluded, that a column of the atmosphere, an inch square, and reaching from the earth to the summit of the atmosphere, weighs between 14 and 15 pounds.

The manner in which this principle acts with reference to the common pump, we will now proceed to detail.

The cylinder, or barrel of a pump, is, in every instance, immersed in water, which is in connexion, by means of springs or other connecting channels, with a reservoir of water open to the action of the atmosphere, which exerts a constant pressure upon its surface: this pressure, by the peculiar nature of fluid bodies before alluded to, is equably diffused throughout the whole extent of the fluid, however numerous may be its ramifying branches; consequently, if one angle, stream, or branch, be for a moment relieved from confinement or pressure, in an opposite or transverse direction, the external force impressed by the atmosphere drives it onwards: this impulse, if communicated either in a downward or a lateral direction, is almost unlimited, for gravitation aids it; but if it be exerted *upwards*, gravitation opposes it, and the limit to its upward motion we will now inquire into.

Fig. 1.

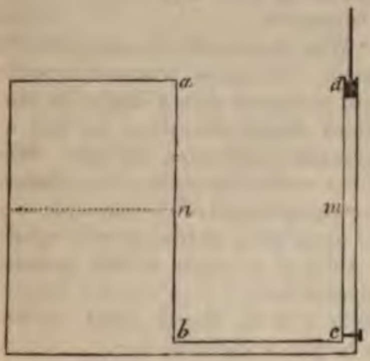


Fig 2.

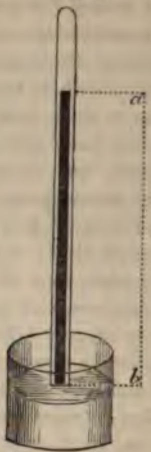
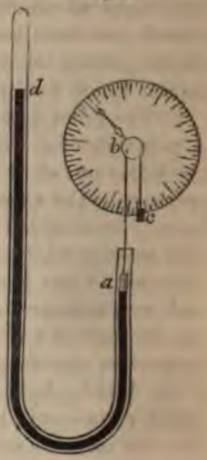


Fig. 3.



Let *a*, fig. 1, represent a reservoir, or cistern, from the lower part of which a horizontal tube, *b c*, springs, which afterwards takes a vertical direction, *c d*; suppose a valve or cock placed at *c*, which, by opening or shutting, admits or prevents the passage of water from the larger vessel to the vertical tube, *c d*, and let the ends, *a* and *d*, be open to the atmosphere, the valve closed, and the reservoir and horizontal pipe full of water: in this position, the valve is pressed on its upper surface, simply by the atmosphere which is resting on it; but on the under surface, the pressure is compounded of the weight of a column of water of the whole height *a b*, and of the atmosphere pressing on that column; the former source (the liquid pressure) being derived from the obvious circumstance that, in a body of water, as well as every other fluid, liquid or gaseous, each fluid stratum bears a greater weight and pressure than the stratum next above it, and, therefore, the water in the horizontal tube has to bear a great pressure at its left extremity, from this cause alone, and when aided by the atmospheric pressure on the water in the cistern, gives a combined power, acting on the lower surface of the valve, much greater than that sustained by the upper surface; the consequence of which necessarily is, that, on opening the valve, the water rushes up the small pipe, by virtue of the greater pressure in that direction, and the condition of equilibrium is attained when the water is at the same level in both tubes; and the reason of this may be thus explained: no communication can take place between the reservoir and the vertical tube, excepting through the horizontal pipe, and any progressive motion in the water in the tube is determined by the balance of pressure at its *two extremities*, *b* and *c*; now those two pressures are equal when the *heights of the* water in the other two tubes are equal, and when that equality of height is attained, equilibrium throughout the whole liquid results. It might be supposed by some, that the larger *bulk* of water would exert the greater pressure, and that, therefore, when the heights were equal, the point *b* would bear a greater burden than *c*; but this is prevented by the remarkable property of the equalization of pressure in fluids, by which every particle bears a share in the burden of any additional pressure; and as the *source* of this pressure is from above, downwards (being the result of gravitation), the amount of pressure on any given space at or near the bottom, is measured solely by the *vertical* height of the fluid above it, without regard to its *bulk;* in other words, the pressure on the entire bottom of the vessel, is estimated by the vertical and lateral dimensions, conjointly, of the water above it, but the pressure on any *given spot* is measured by the height only.

Now this is the universal principle by which fluids always attain the same level, if in communication with each other. The even surface of a lake,—the rise of the water in a ship when a hole is broken in the hull,—the supply of a city with water from an elevated reservoir,—all spring from the same cause, viz., the balance of *vertical* fluid pressure (aqueous, or aerial, or both) at any two points.

These, then, being the steps by which liquid bodies attain equilibrium, and these the conditions which determine its continuance, we will now inquire into the effects which a vacuum produces on this equilibrium.

Suppose (referring again to fig. 1) there be sufficient water to fill only a part of the large vessel, the valve at *c* being open, and the water at equal heights in the reservoir and the vertical tube; let an air-tight piston, *d*, with a valve opening upwards, be now fitted to the small tube, and capable of working vertically in it, and sunk to the surface of the water therein at *m;* upon raising the piston by means of an attached rod, the air resting upon it is carried up with it, none of which can again penetrate below the piston, because the valve opens upwards. When, therefore, the piston is raised to the top, we get a vacant space between it and the surface of the water at *m*, and the atmospheric pressure is entirely removed from the latter thereby. This at once disturbs the equilibrium of every particle of water throughout the apparatus; that at *b* is pressed by the aqueous column, *n b*, and by the air above it, while *c* is pressed out by the aqueous column, *m c*, and, in obedience to this unequal pressure, a movement begins from *b* to *c*, until the water has ascended to such a height in *c d*, that the increased aqueous pressure at *c* shall equal the combined aqueous and atmospheric pressure at *b*, and when that equipoise is attained, no further rise will take place in *c d*, whether there be still a portion of vacuum above the water or not.

Now, as the principles on which nature acts are as equally constant on a large as on a small scale, this figure would equally avail for a table apparatus or for a large lake, with streams flowing from its bed; and, viewed in the latter light, we at once come to the principle of the common pump, and to the extremely important consequences which Torricelli deduced therefrom.

The well into which the barrel of a pump dips may, in all cases, be represented by the small tube *c d*, as it is invariably in connexion, by horizontal or inclined channels, such as *a b*, with a reservoir of water of some sort, the fluid level of which determines the height to which the water will rise in the well. If that level be at some mountainous tract above the general level of the ground near the well, the water will rush out of the latter, and form a fountain; if the reservoir be a neighbouring lake on the same level, the well will be filled to the top, but without running over; but in most instances the source is an aggregation of small streams, which collect and form a reservoir below the general surface of the earth, and at whatever depth that may be, the water in any fissure connected with it will be at an equal depth from the ground; and as wells are nothing more than such fissures either natural or artificial, the depth to which we must apply our apparatus for raising the water in the well will be equally determined. This being premised, we can now conceive the small tube *c d* to represent a pump-barrel, in which the water rises to *m*, by being in connexion with a reservoir, *a b*, filled to *n*, through the medium of an underground channel, *b c*. The piston need not be thrust down to *m*, for if it work through a small range of distance near the top, *d*, it will extract the air by small quantities at a time: at each step, the quantity of air in the barrel, *d m*, becomes less, and as its elasticity makes every part of the remaining air share the effects of the resulting expansion, the density and pressure become less than that of the external atmosphere,—the underground channel becomes pressed unequally at its two extremities,—a movement takes place towards *c*, and the water gradually rises in the barrel: this it does to an extent of *thirty-three feet* above *m*, after which no working of the piston will give a greater elevation.

This is the rationale of all "lifting" pumps, as they are erroneously called. The water will ascend through 33 feet of vacuum, merely because the pressure of the external air will exactly balance such a column of water. If the density of the atmosphere were greater than it is, a greater column of water could be supported by it; it follows, therefore, that if a common pump were capable of being used at the bottom of the tin mines in Cornwall, some of which are nearly 2000 feet deep, it would be found that a greater column of water could be supported by the air than at the top of the shaft, on account of the increased density of the air. Again, if we were enabled to work a pump at the top of a mountain 4000 feet high, we should find that the water would rise but 27 feet in the barrel, on account of the decreased density of the air.

It might be thought unnecessary thus to go into the principles of the common pump to prove the principle of the barometer's action; but the connexion between the phenomena is so great, the rationale of the one is so elucidative of that of the other, and Torricelli's investigation was so entirely suggested by the consideration of the action of a pump, that we have deemed the details into which we have entered as a proper part of our subject.

The tube of mercury prepared by Torricelli, as we have described, being a correct indicator of the atmospheric weight or pressure on the open mercury in the basin, was thence called a *Barometer* (from two *Greek* words, *μετρον*, *measure*, and *βαρος*, *weight*). The term is, *perhaps, not* judiciously chosen, for a "measurer of weight" might equally apply to the common balance, steel-yard, &c., while the barometer can only measure fluid pressure.

The same process always produced nearly similar results, and the fact became established on a solid and enduring basis, that the atmospheric pressure is equal to that of *about* 30 inches of mercury; but it was soon found that the mercury was not always of equal height in the same tube, sometimes ascending to nearly 31 inches, and at other times descending to not much above 28; and the inquiry into the causes of this difference may be almost said to have laid the foundation of the study of meteorology, which 200 years of observation and experiment have scarcely sufficed to raise to the rank of a science; for the laws which regulate the fluctuation of the atmosphere are not yet reduced to an available code: we are, up to the present day, merely pioneers, preparing the way for future operations: an immense store of facts have been collected, but the time for digesting them into one harmonious whole, and accounting for the whole by one connected chain of induction, is not yet arrived: we will, therefore, shortly state the principal facts connected with the rise and fall of the mercury in the barometer, without attempting to reduce them to a systematic arrangement; but, before doing so, it may be as well to describe the different modifications of the barometer, leaving the cause of its fluctuations to the next section; and this we do because the barometer, unlike many other instruments, measures an *effect*, the causes of which are yet the subject of inquiry. The thermometer, for instance, measures the changes of temperature in the air: it is higher in Summer than in Winter, at noon than at night, and we can tell *why* it is so, because the solar rays have greater influence at those times; but by the barometer, although the pressure of the air is measured, yet we cannot tell, in the present state of our knowledge, *why* that pressure is greater in February than in October,—at nine in the morning, than at nine at night. These fluctuations will, therefore, be properly considered afterwards.

The essential part of every barometer is a glass tube, 33 or 34 inches long, containing mercury; all the other appendages are merely conveniences for indicating more accurately the height of the mercury. In filling this tube, many precautions are necessary. There is a thin, enveloping film of air clinging to the surfaces of almost all bodies, and, among the rest, to the glass tube and the mercury. Now, this film must be removed, or else it will interfere with the formation of the vacuum; and this end is effected by boiling the mercury in the tube, by which means the air is liberated and driven off; the tube is then, while completely full of hot mercury, inverted into a basin or reservoir of mercury, exposed to the atmosphere. The mercury in the tube then sinks until its height exactly balances the atmospheric pressure on the mercury in the basin: this height, *a b*, fig. 2, varies in this climate from about 28 to 31 inches.

Now the exact difference in this height at any two periods would accurately measure the difference in the counteracting pressures, were it not that whenever *a fall* occurs in the tube a *rise* necessarily takes place in the mercury in the reservoir, on account of the communication between the two—for instance, if the distance from a to b were 28 inches at one time and 30 at another, it would not do to estimate the increase in the pressure of the atmosphere at exactly two inches, for the level *a*, which is the zero of our scale of measurement, is *lower* at the latter than at the former observation, on account of more mercury having left it to ascend the tube; the result is, that although the two levels are two inches further apart than before, the increase of atmospheric pressure is *less* than equivalent to two inches of mercury: it becomes necessary, therefore, to take the comparative areas of horizontal sections of the reservoir and of the tube, and to make an allowance for this circumstance in any deductions from observation.

Whatever may be the causes which vary atmospheric pressure, and consequently vary the height of the mercurial column, it is desirable to attain as much accuracy as possible, in observing changes in that height, and various contrivancies have been suggested for that purpose. In the common wheel barometer (or weather-glass, as it is frequently termed), the tube, instead of terminating at the bottom in a cistern, is turned up again, fig. 3, as a siphon tube, the shorter leg of which is open to the atmosphere. Now, a rise in the longer or closed tube is equivalent to a fall in the shorter one, and *vice versâ*; on the mercurial surface, therefore, in the shorter tube is placed a weight connected with a string which passes over a pulley, on the other side of which it is balanced by another weight; the pulley is furnished with an index or hand, behind which is a circular face (analogous to a clock-face), which is graduated in any manner we please.

When, then, an increase of atmospheric pressure takes place, it depresses the mercury in the short tube, together with the weight *a*; this gives a small revolving motion to the pulley and to the index attached thereto, and the number of degrees passed over by the index denotes the amount of the fall in the mercury: how far this apparatus deserves the name of a weather-glass, we will consider hereafter.

Soon after the invention of the barometer, many suggestions were made, for the purpose of increasing the delicacy of its indications, or for making it more convenient for use; the siphon form has the latter convenience, but its indications are not so delicate as in Torricelli's, because a change of pressure such as would make an inch in the mercurial column of the latter would show but half an inch in the siphon. It is true the two surfaces *a* and *d* would be an inch further apart, but that inch would be compounded of a rise of half an inch at *d*, and a similar fall at *a*, the two parts of the tube being the same in diameter: our unit of measure therefore becomes twice as great, and necessarily decreases in utility.

It was then proposed that a siphon arrangement should be partly filled with mercury, and partly with water or oil, which being lighter than mercury, would show a greater rise and fall with any given change of pressure; but it was found that air and vapour escaped from the oil into the vacuum more than when mercury alone was used, and that when *water* formed any part of the apparatus, the attraction between it and glass always caused some to cling to the latter, when a fall in the tube occurred; and these objections were found more than to counterbalance any advantages derivable from the more extensive rise and fall.

As an inclined line between two different heights is longer than a vertical line, it has been proposed to make the tube *oblique*, so that any vertical rise and fall in the mercury is indicated by a somewhat longer path, but increase of friction, and other causes, render this not very available; and it is a remarkable circumstance in the history of the barometer, that the one first constructed by Torricelli possesses on the whole more useful properties, and fewer defects, than any subsequently proposed. We could, perhaps, scarcely name another instrument of which the same could be said: there have, indeed, been others proposed, of which the *principle* is decidedly better, but requiring a degree of mechanical nicety in the construction such as we have no chance of obtaining.

Some modifications have been adopted for particular purposes,—for instance, one by Mairan, in which the tube is less than 28 inches long, and in which, consequently, no vacuum can exist until the atmosphere is reduced to great rarity; this is used to denote the degree of exhaustion produced by the air-pump in a receiver.

The superior attraction between mercury and other metals to that existing between mercury and glass (indeed, it is said that no attraction exists between the two latter, but, on the contrary, a repulsion,) has been taken advantage of to keep the lower level of the Torricellian vacuum always at the same height. The cistern, fig. 2, is closed at the top, excepting a small space round the tube; this top is fitted on when the mercury in the cistern is at the lowest, and with such a slight degree of pressure as will force a small quantity up through the open space, which will form a liquid mound round the base of the tube. Now, when a decrease of atmospheric pressure causes a descent in the tube, an additional portion of mercury exudes from the open space; but the repulsion between it and the glass prevents it from climbing, as it were, up the outside of the tube, but it spreads out laterally over the cover of the cistern: thus this little liquid mound retains the same elevation during the rise and fall in the tube, and its surface is made the zero of the scale of vertical measurement.

This same object (the maintaining a constant level for the zero of the scale,) is likewise attained by an ingenious contrivance of Fortin's, in which the lower level of the mercury is, at each observation, brought into coincidence with the zero of the scale by means of a screw which elevates or depresses the cistern containing it.

There are considerations connected with the comparative compression of different liquids by the same force, which have led to the construction of a *water barometer*, with the hope that its greater range of rise and fall would measure more minute changes of pressure; but such a barometer, necessarily 33 feet long, is out of the range of common experiment. There is, however, a very capital one in the Royal Institution, constructed by Mr. Daniel, and another at the College in Edinburgh, constructed by Mr. Adie.

There is yet another form to be briefly noticed,—the marine barometer, "which differs from that used on shore, in having its tube contracted in one place to a very narrow bore, so as to prevent that sudden rising and falling of the mercury, which every motion of the ship would else occasion."—*Arnott.*

These are the principal modifications which have been proposed in the construction of the barometer, with the view of increasing the indicative power; but for philosophical purposes there are many considerations of great delicacy to be attended to.

Mercury and glass, in common with all other bodies, expand by heat, and therefore a rise in the barometric mercury is not absolutely due to increased pressure, for a rise in temperature in the surrounding air, independent of an increase in its pressure, will expand, and therefore elevate, the mercury, and not only so, but the graduated scale, which is generally attached to a barometer tube, expands also. But unless an equal degree of expansion takes place in all the parts of the instrument with a given increase of temperature, a discrepancy in the result will occur. Now, it is found that solid metals (such as the scale is made of,) and mercury and glass, expand unequally, and it is necessary to apply mathematical formulæ to correct the difference; for common purposes, however, the fact that mercury expands much more than the metal of the scale, has led to the neglect of the expansion of the latter.

Another circumstance is the effect of capillarity, or the manifestation of attraction or repulsion between a solid and a fluid, when the latter is contained within a very small tube of the former. Now, if we employed water as a balance to the atmospheric pressure, capillary attraction would raise the water somewhat higher in the tube than would be due to the pressure alone; but as the bias between mercury and glass is of a repulsive nature, the mercurial column is somewhat *lower* than it should be: corrections for this effect have likewise been deduced mathematically.

Much uncertainty exists as to the real nature of the causes which render the surface of the mercury in the barometer tube *convex* when the mercury is rising, and *concave* when it is falling. Some have considered that the concave form while sinking indicates an attraction rather than a repulsion between the glass and the mercury; while a writer in the *Encyclopædia Britannica*, article "Barometer," states that if the mercury were absolutely pure, and every trace, every atom of moisture and air removed both from the mercury and the inside of the tube, that the surface would be *always* convex, the concavity being due to the presence of particles of bodies which have an attraction for glass: this subject requires and deserves further investigation.

The last source of error which we will mention, is the existence of mercurial vapour in what we have termed the vacuum. It has been stated that evaporation goes on from any fluid until an atmosphere of the same kind exists above it, the tension of which prevents more from forming; but there is no possibility of getting rid of this mercurial atmosphere, and therefore, the smaller the excess of the tube's length above that of the mercurial column the better.

The scale attached to the barometers is graduated into 10ths of an inch, but by the application of a small moveable scale called a *Vernier*, from the name of the inventor, a rise or fall of $\frac{1}{100}$th of an inch can be accurately read off.

A POPULAR COURSE OF GEOLOGY.

IX.

Chemical Characters of Minerals (*Continued*).

Metals.

For the reasons assigned in our last Number, we now proceed to describe the behaviour of most of the metals towards reagents, both in a state of solution, and before the blowpipe.

Manganese = 28. Specific gravity 6·85.

Manganese is a hard, brittle, granular, and greyish-white metal, not attracted by the magnet when pure, highly infusible, soon tarnishing on exposure to the air, and absorbing oxygen rapidly when heated to redness in open vessels. It likewise decomposes water at a red heat, hydrogen and protoxide of manganese being the products. From its great affinity for oxygen, it is never met with in the metallic state, but the peroxide is an abundant mineral production.

There is some doubt about the number of oxides of manganese, but the existence of three is well established. The protoxide forms the base of the common salts of manganese. When pure, it is of a light green colour, but, by the gradual absorption of oxygen, changes to brown. It is characterized by the following behaviour with reagents.

1. *Pure Potassa.*—This precipitate is the hydrate of the protoxide, which quickly becomes oxidized by the action of the air, its colour changing to yellowish, then to brown, and finally to black.

2. *Pure Ammonia.*—In neutral solutions a white precipitate, which changes to brown, and, at the point of contact with the air, to black. A solution of muriate of ammonia prevents the precipitate, and re-dissolves it when formed, but the clear solution becomes brown by absorption of oxygen, and deposits the insoluble black deutoxide.

3. *Carbonate of Potassa.*—A white precipitate which does not change colour by exposure to the atmosphere, and is slightly soluble in a solution of muriate of ammonia.

4. *Bicarbonate of Potassa.*—A white precipitate which, in dilute solutions, does not appear for some time.

5. *Carbonate of Ammonia.*—A white precipitate not affected by the action of the air, and very slightly soluble in muriate of ammonia.

6. *Phosphate of Soda.*—A white precipitate unaffected by the action of the air.

7. *Oxalic Acid.*—Crystals of oxalate of manganese in concentrated neutral solutions, but not in those that are too dilute. They disappear on the addition of nitric or muriatic acid. Solutions of the oxalates produce the same crystalline precipitates.

8. *Prussiate of Potassa.*—A white or pale red precipitate, soluble in free acids.

9. *Red Prussiate of Potassa.*—A brown precipitate, not soluble in acids.

A TABLE *of the Action of Reagents on Solutions of some of the Metallic Oxides and their Salts.*

	MANGANESE.		ZINC. —	CADMIUM. —	COBALT. —	NICKEL. —	LEAD.		LEAD. —	BISMUTH. —	COPPER. —
	Protoxide.	Peroxide.	Oxide.	Oxide.	Protoxide.	Protoxide.	Protoxide.	Peroxide.	Protoxide	Oxide.	Peroxide.
Pure or Caustic Potassa	1 white changes to black	1 dark brown	1 white and gelatinous	1 white	1 blue	1 apple green	1 white ch. to brown	1 reddish brn.	1 white	1 white	1 blue
Pure or Caustic Ammonia	2 white ch. to black	1 dark brown	1 white and gelatinous	2 white	2 blue	2 blue solution	2 white ch. to brown	1 reddish brn.	2 white	1 white	2 blue solution
Carbonate of Potassa	3 white	2 brown	2 white	3 white	3 red	apple green	3 white	2 reddish brn.	3 white	1 white	3 blue
Bicarbonate of Potassa	4 white	2 brown	2 white	3 white	3 red	apple green	3 white	2 red brown	3 white	1 white	4 light green
Carbonate of Ammonia	5 white	2 brown	3 white	3 white	4 red	3 blue solution	3 white	2 red brown	3 white	1 white	2 blue solution
Phosphate of Soda	6 white	3 brown	4 white	4 white	5 blue	4 greenish wht.	4 white ch. to green	3 white	4 white	white	5 greenish wht.
Oxalic Acid	7 crystals	4 —	5 white	5 white	6 white	5 green	5 yel. crystals	4 —	5 white	2 crystals	6 greenish wht.
Prussiate of Potassa	8 white or pale red	5 greyish green	6 white and gelatinous	6 white	7 green	6 white	6 white or light blue	5 dark blue	white	3 white	7 reddish brn.
Red Prussiate of Potassa	9 brown	6 brown	7 yellowish red	7 yellow	8 reddish brown	7 yellowish green	7 dark blue	6 —	—	4 pale yellow	8 yellowish grn.
Hydrosulphuret of Ammonia	10 flesh red	7 flesh red	8 white	8 yellow	9 black	8 black	8 black	7 black	6 black	5 black	9 black
Liquid Sulphuretted Hydrogen	11 —	8 white	9 white	9 yellow	10 black	9 black	9 —	8 white	7 black	6 black	10 black
Sulphuretted Hydrogen Gas	11 —	8 white	9 white	9 yellow	10 black	9 black	9 —	8 white	7 black	6 black	10 black
Sulphuric Acid, and Soluble Sulphates	—	—	—	—	—	—	—	—	8 white	—	—
Muriatic Acid, and Chlorides	—	—	—	—	—	—	—	—	9 white	—	—
Iodide of Potassium	—	—	—	—	—	—	—	—	10 yellow	7 brown	11 white
Chromate of Potassa	—	—	—	—	—	—	—	—	11 yellow	8 yellow	12 reddish brn.
Water	—	—	—	—	—	—	—	—	—	9 milkiness	—
Precipitated in the metallic state by metallic	—	—	—	zinc	—	—	—	—	zinc	zinc	zinc and iron
Before the blowpipe colour glass of borax & microcosmic salt	amethyst *	amethyst	milky	colourless	blue	colourless	green	green	colourless	grey	bluish green

* For the variations of colour in the outer or inner flame, and while the bead is hot or cold, see the subsequent pages under the head of each metal.

10. *Hydrosulphuret of Ammonia.*—A yellow or flesh-red precipitate, but the colour cannot be judged of till the precipitate has completely subsided. It is insoluble in an excess of the precipitant. Contact of the air changes its colour to brown.

11. *Liquid Sulphuretted Hydrogen, and Sulphuretted Hydrogen Gas.*—No precipitate, except when the protoxide of manganese is combined with a very weak acid. A few drops of ammonia, after adding the sulphuretted hydrogen, will produce a flesh-red precipitate.

Solutions of protoxide of manganese are distinguished from solutions of alkaline and earthy salts by giving a precipitate with hydrosulphuret of ammonia.

Before the blowpipe, with *borax* or *microcosmic* salt, the protosalts of manganese in the outer flame, tinge the bead of an amethystine colour, which disappears in the inner flame, and reappears in the outer.

With *soda* on platinum foil, a very minute portion may be detected by the pale green colour which the fused mass acquires.

DEUTOXIDE OF MANGANESE.

THE deutoxide, when pure, is black; digested in sulphuric acid, it forms a violet-coloured solution. In muriatic acid it dissolves, with a slight disengagement of chlorine, even in the cold, but more quickly, and with a stronger smell of chlorine, when boiled. The solution formed by cold muriatic acid is dark brown, but it loses its colour on being boiled. In the muriatic solution, the following effects are produced by reagents.

1. *Pure Potassa and Pure Ammonia.*—Both occasion dark brown voluminous precipitates, the production of which is not hindered by muriate of ammonia.

2. *Carbonate and Bicarbonate of Potassa and Carbonate of Ammonia.*—A brown voluminous precipitate.

3. *Phosphate of Soda.*—In the acid muriatic solution, accurately neutralized by ammonia, a brown precipitate, more voluminous, and of a lighter colour, than that produced by the above reagents.

4. *Oxalic Acid.*—No precipitate, but after some time the liquor is rendered colourless.

5. *Prussiate of Potassa.*—A greyish-green precipitate.

6. *Red Prussiate of Potassa.*—The same brown precipitate as in solutions of the protoxide.

7. *Hydrosulphuret of Ammonia.*—In a solution saturated with ammonia, a flesh-red precipitate, like that of the protoxide.

8. *Liquid Sulphuretted Hydrogen and Sulphuretted Hydrogen Gas.*—A white precipitate, which is sulphur; and the solution becomes a solution of the protoxide.

Before the blowpipe, deutoxide of manganese and its compounds have the same action on fluxes as the salts of the protoxide.

PEROXIDE OF MANGANESE.

THIS is the common ore, known by the name of black oxide of manganese. It generally occurs massive, but sometimes in the form of stellular groups of prismatic crystals. It is insoluble in water, and does not unite with acids or alkalies; but when boiled with sulphuric acid, a sulphate of the protoxide is formed, and oxygen gas is set free. When boiled with muriatic acid, muriate of the protoxide is formed, with disengagement of chlorine. The solution in both cases is of a deep red colour. The peroxide is converted by ignition, at a red heat, into the deutoxide, with evolution of oxygen gas. The hydrate of the deutoxide, which likewise occurs native, is often mixed with, and in commerce mistaken for, the peroxide. It gives a brown streak on unglazed porcelain, whereas that of the protoxide is black. The hydrate of the deutoxide gives off water when heated in a small glass tube closed at one end, which the peroxide, when pure, does not.

ZINC = 34. Specific gravity 7.

THE most common ores of zinc are the sulphuret or *black-jack*, and the carbonate or *calamine*. Metallic zinc has a strong lustre, a bluish-white colour, and laminated structure. It is brittle at low or high degrees of heat, but at an intermediate temperature is malleable and ductile. After fusion, it assumes regular forms when slowly cooled. It sublimes unchanged when exposed to a white heat in close vessels. Air and moisture have little effect in oxidizing zinc. Fused in open vessels, it absorbs oxygen, and forms the white oxide or flowers of zinc. At a full red heat, in contact with air, it burns with a brilliant white light and production of oxide. It is likewise easily oxidized by dilute nitric or muriatic acid, with evolution of hydrogen, derived from the water of the acid. Chemists are acquainted with but one oxide of zinc. In a pure state it is white, becomes yellow by heat, but resumes its white colour when cold. It is insoluble in water, but is soluble in acids, with which it forms for the most part colourless salts. It likewise combines with some of the alkalies.

OXIDE OF ZINC.

1. *Pure Potassa and Ammonia.*—The hydrate, a white gelatinous precipitate, soluble in an excess of the precipitant.

2. *Carbonate of Potassa.*—A white carbonate of zinc, insoluble in an excess of the precipitant, but soluble in a solution of potassa or ammonia.

3. *Carbonate of Ammonia.*—A white precipitate, soluble in an excess of the precipitant.

4. *Phosphate of Soda.*—In neutral solutions a white phosphate, soluble in acids, potassa, and ammonia.

5. *Oxalic Acid.*—In neutral solutions a white oxalate, which increases after a time, and is soluble in potassa, ammonia, and acids. In very dilute solutions a troubling appears after some time, but no precipitate is produced. Binoxalate of potassa has the same effect as oxalic acid.

6. *Prussiate of Potassa.*—A white gelatinous precipitate, insoluble in free muriatic acid.

7. *Red Prussiate of Potassa.*—A yellowish-red precipitate, soluble in free muriatic acid.

8. *Hydrosulphuret of Ammonia.*—In neutral solutions a white precipitate (sulphuret of zinc), insoluble in an excess of the precipitant. The presence of iron in small quantities renders this precipitate grey, in larger quantities, black.

9. *Liquid Sulphuretted Hydrogen and Sulphuretted Hydrogen Gas.*—A white precipitate in neutral solutions, but none in those that are acid.

Solutions of salts of zinc (and those that do not dissolve in water are soluble in diluted sulphuric or muriatic acid,) are distinguished from solutions of alkaline salts, by yielding a precipitate with carbonate of potassa. From the earthy salts they are distinguished by their white precipitate with hydrosulphuret of ammonia, even in alkaline solutions in which the precipitate formed by potassa or ammonia has been dissolved in an excess of those precipitants. Salts of alumina (see ALUMINA) yield no precipitate with hydrosulphuret of ammonia under such circumstances. A white precipitate produced by hydrosulphuret of ammonia, in a clear and strong alkaline solution, can be nothing else than sulphuret of zinc.

Before the blowpipe, compounds of zinc are easily detected by the white coating of oxide of zinc which they spread on the charcoal when heated. They assume a green colour when moistened with nitrate of cobalt, and heated on charcoal.

With *borax*, or *microcosmic salt*, oxide of zinc melts into a clear glass, which flaming renders milky.

With *soda* it does not melt, but, in the interior flame, is reduced and burns with its characteristic flame, depositing a white coating on the charcoal. Mixed with oxide of copper, the two oxides are reduced, and form an alloy, which is brass.

CADMIUM = 56. Specific gravity 8·604.

CADMIUM is a very rare metal, and we should therefore pass it over had it not been found in most of the British ores of zinc. It was discovered in 1817 by Stromeyer. In colour and lustre, metallic cadmium resembles tin, but has greater hardness and tenacity*, with the same degree of fusibility. It is both ductile and malleable, and is nearly as volatile as mercury, condensing into globules which have a metallic lustre. Its vapour is inodorous.

When heated in open vessels, cadmium is converted into an oxide. It is also easily oxidized by nitric acid, which is its proper solvent, and less readily by sulphuric and muriatic acid. There is but one known oxide of cadmium, which is not volatilized until mingled with charcoal powder, when it is reduced to metallic cadmium, which, as we before observed, is highly volatile. The oxide is of an orange colour, insoluble in water, but forming with acids a strong salifiable base.

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* The relative tenacity of metals is ascertained by trying the weight which wires of the same thickness will sustain without breaking.

OXIDE OF CADMIUM.

1. *Pure Potassa.*—A white hydrate, insoluble in an excess of the precipitant.

2. *Pure Ammonia.*—A white hydrate, easily soluble in a slight excess of the precipitant.

3. *Carbonate and Bicarbonate of Potassa and Carbonate of Ammonia.*—White precipitates, insoluble in an excess of these precipitants.

4. *Phosphate of Soda.*—In neutral solutions, a white precipitate.

5. *Oxalic Acid.*—In neutral solutions, an immediate white precipitate, easily soluble in ammonia.

6. *Prussiate of Potassa.*—A white precipitate, slightly inclining to yellow, soluble in muriatic acid.

7. *Red Prussiate of Potassa.*—A yellow precipitate, likewise soluble in muriatic acid.

8. *Hydrosulphuret of Ammonia.*—In neutral solutions, a yellow precipitate, insoluble in an excess of the precipitant.

9. *Liquid Sulphuretted Hydrogen and Sulphuretted Hydrogen Gas.*—A yellow precipitate, both in neutral and acid solutions.

By these last two yellow precipitates, *insoluble* in an excess of the precipitants, oxide of cadmium in solution is distinguished from all other substances. Peroxide of tin and arsenious acid likewise give yellow precipitates with hydrosulphuret of ammonia, but they are *soluble* in an excess of it.

Before the blowpipe, cadmium may be detected in its compounds, by the orange-coloured powder which they deposit on the charcoal when heated with soda.

With *borax*, oxide of cadmium forms a glass, yellow while hot, colourless when cold.

With *microcosmic salt*, it forms a glass, clear while hot, colourless when cold.

Cadmium is precipitated in the metallic state from its solutions, by a bar of *zinc*, in the form of grey-coloured spangles.

COBALT = 26. Specific gravity 8·538.

COBALT is a brittle metal, of a greyish colour, and weak metallic lustre, attracted by the magnet, and capable of being rendered permanently magnetic. Cobalt generally accompanies meteoric iron. Its principal ore is the arseniuret. When this, reduced to small pieces, is exposed to heat in a reverberatory furnace, it gives off vapours of arsenious acid and *zaffre* of commerce; an impure oxide of cobalt remains. This, fused with a mixture of sand and potassa, produces a blue glass, which, reduced to powder, is known in commerce by the name of *smalt*.

Cobalt is but slightly acted on by air and moisture, but absorbs oxygen when heated in open vessels. It is also readily oxidized by nitric acid, and, with more difficulty, by sulphuric and muriatic acid. There are two oxides of cobalt. The protoxide, in its pure state, is of a greenish-grey colour. It dissolves in acids, forming the base of the salts of cobalt, which are for the most part pink. With muriatic acid, it sometimes gives out chlorine, from containing a portion of the peroxide.

PROTOXIDE OF COBALT.

1. *Pure Potassa.*—A blue precipitate, the hydrate, which turns green after some time, or when collected on a filter, and pale red when boiled. It is insoluble in an excess of the precipitant.

2. *Ammonia.*—This precipitate, which at first is blue, is changed to green by further addition of ammonia; a still larger quantity of which dissolves most of the precipitate, producing a greenish solution, which, by exposure to the air, gradually changes to a dark brown. The presence of muriate of ammonia in a solution of protoxide of cobalt, prevents a precipitate by pure ammonia, but the solution, after a time, acquires a brown colour.

3. *Carbonate of Potassa.*—A red precipitate, which changes to blue on being boiled.

4. *Carbonate of Ammonia.*—A red precipitate, which dissolves in muriate of ammonia, producing a red liquor, which does not change to brown. The presence of muriate of ammonia prevents any precipitate.

5. *Phosphate of Soda.*—In neutral solutions, a blue precipitate.

6. *Oxalic Acid.*—In neutral solutions, but not until after some time, a precipitate which is white, or only very slightly reddish.

7. *Prussiate of Potassa.*—A green precipitate, which changes to grey, and is insoluble in muriatic acid.

8. *Red Prussiate of Potassa.*—A dark reddish-brown precipitate, insoluble in muriatic acid.

9. *Hydrosulphuret of Ammonia.*—In neutral solutions, a black precipitate, insoluble in an excess of the precipitant.

10. *Liquid Sulphuretted Hydrogen, and Sulphuretted Hydrogen Gas.*—In neutral solutions, no immediate precipitate. After a time, the colour of the solution changes to blackish-brown, and a slight precipitate take place, of a black colour. In acid solutions there is no trace of a precipitate, even after a considerable time.

Solutions of oxide of cobalt are distinguished by their behaviour with hydrosulphuret of ammonia and sulphuretted hydrogen gas. Whenever an acidulated solution yields no precipitate with the latter, while in a neutral or alkaline solution a black precipitate is formed with hydrosulphuret of ammonia, it can scarcely contain any other metal than cobalt, nickel, or iron; and cobalt is distinguished from the other two by the dark-blue colour which the smallest portion of its salts imparts to the glass of borax or microcosmic salt before the blowpipe.

PEROXIDE OF COBALT.

THIS oxide is of a black colour; disengages oxygen gas when strongly heated, and dissolves in concentrated and boiling muriatic acid, with disengagement of chlorine. *Before the blowpipe*, it forms the same dark-blue glass as the protoxide with borax and microcosmic salt.

NICKEL = 26. Specific gravity 8 to 9.

NICKEL is of a white colour, between that of tin and silver; has a strong metallic lustre, is ductile and malleable, is attracted by the magnet, and may be rendered permanently magnetic. It is usually a constituent of meteoric iron. It is also found in a mineral from Westphalia, called copper nickel (on account of its resemblance to copper), which is an native arseniuret of nickel, containing likewise sulphur, copper, cobalt, and iron. *Speiss* is an artificial arseniuret of nickel, obtained in preparing smalt from the roasted arseniuret of cobalt.

Nickel is more infusible than iron, and does not oxidize by exposure to the atmosphere at common temperatures, but at a red heat absorbs oxygen, though not rapidly. The muriatic and sulphuric acids act upon it with difficulty. Nitric acid easily dissolves it, forming a nitrate of the protoxide. There are two oxides of nickel. The protoxide, when pure, is of a dark-grey colour; it dissolves in acids, forming green-coloured solutions, which behave towards reagents like those salts of nickel which are soluble in water.

PROTOXIDE OF NICKEL.

1. *Pure Potassa.*—The hydrate, of an apple-green colour, not soluble in an excess of potassa.

2. *Ammonia.*—A small quantity of ammonia produces a slight green troubling, which disappears on the addition of more ammonia. The clear solution now assumes a blue colour, and in this ammoniacal solution, potassa produces an apple-green precipitate.

3. *Carbonate of Ammonia.*—In neutral solutions, an apple-green precipitate, which dissolves in an excess of the precipitant, producing a blue solution.

4. *Phosphate of Soda.*—A white precipitate, tending slightly to green.

5. *Oxalic Acid.*—In neutral solutions, after some time, a greenish precipitate appears, which increases by standing until the solution becomes perfectly colourless.

6. *Prussiate of Potassa.*—A white precipitate, tending slightly to green, and insoluble in muriatic acid.

7. *Red Prussiate of Potassa.*—A yellowish-green precipitate, insoluble in muriatic acid.

8. *Hydrosulphuret of Ammonia.*—In neutral solutions, a black precipitate, nearly insoluble in an excess of the precipitant. The supernatant fluid also acquires a black colour.

9. *Liquid Sulphuretted Hydrogen, and Sulphuretted Hydrogen Gas.*—Neutral solutions acquire a black colour, and, after a long time, a slight black precipitate takes place. Acidulated solutions afford no precipitate, even after a long time.

Solutions of oxide of nickel are distinguished from solutions of all other metallic oxides, except cobalt, by their behaviour with hydrosulphuret of ammonia.

They are distinguished from those of cobalt, by the blue colour imparted to them by ammonia.

Before the blowpipe, salts of nickel communicate to a bead of *borax*, or *microcosmic* salt, a reddish colour, which fades as the bead cools, and often disappears entirely when it is quite cold. With *soda, on charcoal*, they are reduced to a white metallic powder, which is magnetic.

PEROXIDE OF NICKEL.

THIS oxide is of a black colour; gives out oxygen when heated, and dissolves in concentrated muriatic acid, with disengagement of chlorine. Before the blowpipe, its characteristics are the same as those of the protoxide.

IRON = 28. Specific gravity 7·78.

THIS abundant, useful, and well-known metal, has a grey colour, and strong metallic lustre. It is less malleable and ductile than many other metals, but superior to them all in tenacity. It is very infusible, but when heated to redness, becomes soft and pliable, so that it may be beaten into any form; and it possesses another very useful and valuable property, namely, a capability of being *welded*, or incorporated with another piece of red-hot iron by hammering.

Iron is attracted by the magnet, and may be rendered permanently magnetic, properties possessed by no other metals, except nickel and cobalt. Native metallic iron is very rare. That of aërolites is alloyed with cobalt and nickel. The most abundant ores of iron are the oxides, generally mixed with argillaceous and siliceous matter. Iron has a strong affinity for oxygen, and, in a moist atmosphere, is soon covered with *rust*. When heated to redness, it absorbs oxygen rapidly from the air. The scales which then separate from it, consist of the black oxide. It burns with brilliant scintillations in oxygen gas; and at all temperatures, from that of a low red to a white heat, it decomposes the vapour of water, and combines with its oxygen.

There are but two oxides of iron, the *blue*, a protoxide, and the *red*, a peroxide. The *black*, long regarded as the protoxide, is a compound of these two united in variable proportions.

PROTOXIDE OF IRON.

THIS oxide is almost unknown in the pure state, because, when precipitated, it soon becomes more highly oxidized. It is the base of the protosalts of iron, and is produced when that metal is dissolved in diluted sulphuric acid.

1. *Potassa.*—The hydrate, a flocculent precipitate, which at first is nearly white, but soon changes to grey, green, and finally acquires a reddish-brown colour, where it is in contact with the atmosphere.

2. *Ammonia.*—The appearances are the same as with potassa. The presence of muriate of ammonia in the solution, hinders this precipitate, but on exposure to the air, a green precipitate subsides in small quantities, which becomes reddish-brown on the surface.

3. *Carbonate and Bicarbonate of Potassa, and Carbonate of Ammonia.*—The carbonate of iron thrown down by these reagents is white, and does not change colour at the surface so soon as the precipitate formed by pure potassa. It dissolves in a solution of muriate of ammonia, but when this solution stands in contact with the air, a green precipitate is formed, which becomes reddish-brown at the surface.

4. *Phosphate of Soda.*—A white precipitate, which becomes green by long exposure to the air.

5. *Oxalic Acid.*—A yellow colour is immediately communicated to the solution, and, after some time, a yellow precipitate takes place, of protoxalate of iron, which dissolves in an excess of muriatic acid. With neutral alkaline oxalates, the effect is more distinct, and takes place immediately.

6. *Prussiate of Potassa.*—A precipitate insoluble in muriatic acid. At the instant of its production it is white, if the air be excluded; otherwise it is light-blue, and by long exposure to the air it changes to dark-blue.

7. *Red Prussiate of Potassa.*—An immediate dark-blue precipitate, insoluble in acids.

8. *Hydrosulphuret of Ammonia.*—In neutral solutions, a black precipitate of suphuret of iron, insoluble in an excess of the precipitant, and distinguished from the precipitated sulphurets of cobalt and nickel, by becoming oxidized in contact with the air, and changing to reddish-brown.

9. *Liquor Sulphuretted Hydrogen.*—No precipitate in neutral solutions.

The red prussiate of potassa, and hydrosulphuret of ammonia, are tests by which the presence of protoxide of iron is most easily detected.

Before the blowpipe, the protosalts of iron yield with *borax* a glass, which, in the outer flame, is of a deep red, and becomes lighter as it cools. In the inner flame it is green when hot, but colourless when cold, unless a large quantity of protoxide of iron is present.

With *microcosmic salt*, very small quanties of iron impart to the resulting glass a green colour while hot, which fades in cooling, and disappears entirely when the glass is quite cold.

With *soda* on charcoal, the protosalts of iron are reduced, and yield a magnetic powder, after grinding and washing away the light particles of charcoal.

PEROXIDE OF IRON.

IN a state of purity this oxide is red, or reddish-brown. It occurs native as hæmatite and iron glance. The latter is of a grey colour, with a metallic lustre, and yields a red powder when scraped. The peroxide, precipitated from its solutions, is black, but its powder is red. When recently precipitated, it is easily soluble in acids, but becomes less so after ignition; yet, even then, it is completely soluble, particularly in muriatic acid. Most of its salts are red.

1. *Pure Potassa and Ammonia.*—Both these reagents throw down the hydrate of a reddish-brown colour, insoluble in an excess of the precipitant.

2. *The Carbonated Alkalies.*—A precipitate of a reddish-brown colour, but somewhat lighter than that produced by caustic alkalies.

3. *Phosphate of Soda.*—In neutral solutions, a white precipitate, which, on the addition of ammonia, turns brown, and after a time, completely dissolves.

4. *Oxalic Acid.*—No precipitate, but the solution acquires a yellow colour.

5. *Prussiate of Potassa.*—An immediate dark-blue precipitate, insoluble in muriatic acid.

6. *Red Prussiate of Potassa.*—No precipitate, but the solution acquires a darker colour.

7. *Hydrosulphuret of Ammonia.*—In neutral solutions, a black precipitate, insoluble in an excess of the precipitant. It oxidizes by contact with the atmosphere, and changes to a reddish-brown colour.

8. *Liquid Sulphuretted Hydrogen, and Sulphuretted Hydrogen Gas.*—In neutral solutions a milky-white precipitate of sulphur, the solution of the peroxide being converted into a solution of the protoxide.

The most characteristic tests of the presence of peroxide of iron are hydrosulphuret of ammonia, sulphuretted hydrogen, and prussiate of potassa. Before the blowpipe the persalts of iron have the same action on fluxes as the protosalts.

Compounds of the Protoxide and Peroxide of Iron.

These compounds are produced when iron is heated in contact with the air; they also occur native, and are known by the name of magnetic iron ore.

To detect the two oxides, the compound is to be dissolved in *a closed flask* in muriatic acid, and the solution being divided into two portions, to one liquid sulphuretted hydrogen is added, and the presence of peroxide is proved by the white milky precipitate of sulphur; the other portion is treated with the *red* prussiate of potassa, which, by yielding a dark blue precipitate, indicates the presence of the protoxide.

Lead = 104. Specific gravity 11·358.

Lead is of a bluish grey colour, and has a strong metallic lustre when fresh cut, but soon tarnishes. It is soft, flexible, and inelastic. Malleability it possesses in a high degree, but is inferior in tenacity to all the ductile metals. It is easily fusible, and, in close vessels, may be heated to whiteness without subliming. When heated in contact with air, it is soon oxidized, and, by a strong heat, is rapidly dissipated in yellow fumes, which consist of the protoxide. The proper solvent for lead is nitric acid, by which it is rapidly oxidized. The muriatic and sulphuric have scarcely any action on it, and precipitate it as a chloride or sulphate from its solutions. There are three oxides of lead. The protoxide, the *litharge* of commerce, is prepared by exposing the grey film which forms on the surface of melting lead to heat and air, until it acquires a yellow colour. This oxide is insoluble in water, fuses at a red heat, and, in close vessels, undergoes no change in the fire. It is easily reduced to the metallic state by heating it with combustible substances, which abstract its oxygen. It unites with acids, and is the base of all the salts of lead, most of which are white. Nearly all the compounds of lead are poisonous.

Protoxide of Lead.

1. *Pure Potassa.*—The white hydrate thrown down by this reagent dissolves in a considerable excess of the precipitant.

2. *Ammonia.*—A white precipitate, not soluble in an excess of the precipitant. Solutions of acetate of lead are not troubled, even when concentrated, by ammonia; but, after some time, crystals of a sub-salt *are deposited.*

3. *The Alkaline Carbonates.*—A white precipitate of carbonate of lead, insoluble in an excess of the precipitants, but soluble in pure potassa.

4. *Phosphate of Soda.*—In neutral solutions, a white precipitate, soluble in pure potassa.

5. *Oxalic Acid.*—In neutral solutions, an immediate precipitate, which is white.

6. *Hydrosulphuret of Ammonia.*—A black precipitate, insoluble in an excess of the precipitant.

7. *Liquid Sulphuretted Hydrogen, and Sulphuretted Hydrogen Gas.*—A black precipitate, both in neutral and acid solutions.

8. *Diluted Sulphuric Acid, and the Soluble Sulphates.*—A white precipitate, insoluble, or sparingly soluble, in acids, but soluble in pure potassa. This is a very discriminative test for lead, because sulphuric acid produces precipitates insoluble in diluted acids with no substances besides lead and the alkaline earths, baryta, strontia, and lime. The earthy sulphates thus precipitated are not soluble, like that of lead, in a solution of pure potassa, nor do they, like it, assume a black colour when moistened with hydrosulphuret of ammonia.

9. *Muriatic Acid, and the Chlorides.*—In solutions of protoxide of lead, when not too dilute, a white precipitate, which redissolves on the addition of water, and is soluble in a solution of potassa. Ammonia produces a precipitate in the aqueous solution of chloride of lead.

10. *Iodide of Potassium.*—A yellow precipitate, soluble in a great excess of the precipitant.

11. *Chromate of Potassa.*—A yellow precipitate, insoluble in diluted nitric acid, soluble in a solution of pure potassa.

Metallic Zinc precipitates lead from its solutions in the metallic state, as blackish-grey shining spangles. Solutions of protoxide of lead are distinguished from solutions of the oxides of other metals by diluted sulphuric acid, and from the alkaline earths by hydrosulphuret of ammonia. *Before the blowpipe*, lead is discriminated by the ease with which its compounds are reduced with soda on charcoal in the inner flame into metallic globules, easily flattened by the hammer, and not brittle.

With *borax* on the platinum wire it forms a clear glass, yellow while hot, colourless when cold.

With *microcosmic* salt it forms a clear colourless glass.

DEUTOXIDE OF LEAD.

THIS is the minium, or red-lead of commerce, and is formed by heating the protoxide in open vessels, over which a current of air is made to play. When gently heated, it gives out oxygen, and is reconverted into the protoxide. It does not unite with acids. Digested with nitric acid, it is resolved into the protoxide and peroxide; the former combines with the acid, the latter remains undissolved.

PEROXIDE OF LEAD.

THIS is a dark brown powder, obtained as described above, by digesting the deutoxide with nitric acid. When heated, it gives out oxygen, and is converted into the protoxide. With muriatic acid it is converted into chloride of lead, with disengagement of chlorine.

BISMUTH = 72. Specific gravity 10.

BISMUTH is a whitish metal, having a considerable lustre, and a laminated structure, brittle when cold, but malleable while warm. It is very fusible and volatile. Bismuth sometimes occurs native, but is more commonly combined with sulphur, arsenic, silver, and cobalt.

Bismuth undergoes but slight oxidation from exposure to the atmosphere at common temperatures; but, when fused in open vessels, is soon covered with a grey film. At its subliming point it takes fire, and burns with a bluish flame, emitting white fumes of oxide of bismuth. Muriatic acid and sulphuric acid have but slight action upon it; but it is easily oxidized and dissolved by nitric acid. There is but one oxide of bismuth, which is yellow, and becomes darker when heated, but, on cooling, regains its original colour. In a full red heat it fuses to a yellow glass; at a higher temperature it sublimes. With combustible substances oxide of bismuth is easily reduced to the metallic state. It dissolves in acids, and is the base of all the salts of bismuth, most of which are white. Many of the salts of bismuth, the nitrate among others, are decomposed by the action of water into an acid salt, which dissolves, and a basic salt, which is precipitated, communicating a milkiness to the liquid. The addition of nitric acid completes the solution, and the milkiness disappears. The following effects are then produced by reagents:—

1. *The pure and carbonated Alkalies.*—A white precipitate, insoluble in an excess of the precipitant.

2. *Oxalic Acid.*—A crystalline precipitate, not perceptible until after some time.

3. *Prussiate of Potassa.*—A white precipitate, insoluble in muriatic acid.

4. *Red Prussiate of Potassa.*—A pale-yellow precipitate, soluble in muriatic acid.

5. *Hydrosulphuret of Ammonia.*—A black, or, in smaller quantities, a dark-brown precipitate, insoluble in an excess of the precipitant.

6. *Liquid Sulphuretted Hydrogen, and Sulphuretted Hydrogen Gas.*—Even in acid solutions, a black or dark-brown precipitate of sulphuret, easily reduced to metallic bismuth by the blowpipe, with soda on charcoal, in the inner flame.

7. *Iodide of Potassium.*—A brown precipitate, soluble in an excess of the precipitant.

8. *Chromate of Potassa.*—A yellow precipitate, soluble in dilute nitric acid.

Metallic Zinc precipitates bismuth from its solutions, as a black spongy mass.

Bismuth is best detected in its solutions by the milkiness produced when they are diluted with water, and by their behaviour with hydrosulphuret of ammonia. They are distinguished from solutions of lead by not yielding a precipitate with sulphuric acid, and by the precipitate thrown down by pure potassa being insoluble in an excess of the precipitant.

Before the blowpipe, compounds of oxide of bismuth are recognised by the facility with which they are reduced to metallic globules with soda on charcoal, in the inner flame, the charcoal being coated with a yellow powder. These metallic globules are distinguished from lead by their brittleness.

COPPER = 64. Specific gravity 8·78.

COPPER, the only metal, except titanium, that is of a red colour, acquires considerable lustre by being polished. In the metallic state it is by no means uncommon, but its commonest ore, and that from which most of the copper of commerce is derived, is the sulphuret. Copper is ductile, malleable, somewhat less tenacious than iron, hard, non-elastic, and sonorous; more fusible than gold, and less so than silver. Exposed to air and moisture, it soon becomes covered with a green coating, which is the carbonate of the peroxide. At a red heat it absorbs oxygen, and is converted into the peroxide. It is acted upon with violence by nitric acid, more feebly by the muriatic and sulphuric, and not at all by vegetable acids, if the air be excluded.

There are but two oxides of copper. The red, or protoxide, occurs native in red octohedral crystals, often of great beauty. It forms salts with the sulphuric, muriatic, and some other acids, which, attracting oxygen from the atmosphere, are speedily converted into persalts.

The peroxide of copper likewise occurs native as a powder, varying in colour from brown to bluish-black. It is easily reduced to the metallic state by heat and combustible matter. It is insoluble in water, but soluble in acids, and most of its salts have a blue tint. The muriatic solution is of an emerald-green colour. It likewise dissolves in ammonia, yielding a deep-blue solution. The salts of copper are poisonous.

PEROXIDE OF COPPER.

1. *Potassa.*—A voluminous blue hydrate, which, on being boiled in an excess of potassa, turns black, and quickly subsides; the hydrate is decomposed, parts with its water, and is converted into the peroxide.

2. *Ammonia, and Carbonate of Ammonia.*—In small quantities, a greenish precipitate, which re-dissolves in an excess of the precipitant, and produces a blue solution, of a much darker colour than the ammoniacal solutions of nickel.

3. *Carbonate of Potassa.*—In the cold, a blue precipitate, which is rendered black by boiling.

4. *Bicarbonate of Potassa.*—A light-green precipitate, which dissolves in an excess of the precipitant, yielding a light-blue solution.

5. *Phosphate of Soda.*—A greenish-white precipitate, soluble in ammonia; the solution blue. Potassa produces in the ammoniacal solution a black, heavy precipitate.

6. *Oxalic Acid.*—In neutral solutions, an immediate greenish-white precipitate.

7. *Prussiate of Potassa.*—A reddish-brown precipitate, insoluble in muriatic acid.

8. *Red Prussiate of Potassa.*—A yellowish-green precipitate, *insoluble in* muriatic acid.

9. *Hydrosulphuret of Ammonia.*—In neutral solutions, a black, and in small quantities a brown precipitate, insoluble in an excess of the precipitant.

10. *Liquid Sulphuretted Hydrogen, and Sulphuretted Hydrogen Gas.*—A black, or dark-brown precipitate, in both acids and neutral solutions.

11. *Iodide of Potassium.*—A precipitate which is white after the solution containing free iodine is poured off. It dissolves in an excess of the precipitant.

12. *Chromate of Potassa.*—A reddish-brown precipitate, soluble in an excess of the precipitant, forming an emerald-green solution; soluble also in diluted nitric acid.

Metallic Zinc precipitates copper as a black coating. *Metallic iron* precipitates it as metallic copper of its own colour. If the blade of a knife, or a piece of polished iron, be dipped into a solution containing a very slight trace of copper, it will be immediately covered with a film of that metal.

Solutions of copper are distinguished by the blue colour they acquire from ammonia, and their reddish-brown precipitate with prussiate of potassa. They are distinguished from solutions of nickel, by giving a blue instead of a green precipitate, with potassa, and by yielding, with liquid sulphuretted hydrogen, a black precipitate, in acid as well as neutral solutions.

Before the blowpipe, salts of copper, with *borax* and *microcosmic salt*, produce in the outer flame a glass, yellowish while hot; blue-green when cold: in the inner flame, a glass of a dirty reddish-brown colour, with reduction of the metal.

With *soda* on charcoal, salts of copper are reduced, and very minute portions of copper may thus be detected, in the form of metallic particles, after grinding and washing away the charcoal. By this mode, Gahn extracted particles of metallic copper from the ashes of a sheet of writing paper, long before it was suspected that vegetables contained copper.

ON DIETETIC CHEMISTRY.

V.

ANIMAL SUBSTANCES.

THE chemical history of animal substances, considered in reference to diet, is extremely simple, and includes three or four proximate principles, or distinct products, only.

The *ultimate elements* of which these principles consist, are carbon, oxygen, hydrogen, and nitrogen; and, as in vegetable products nitrogen is rarely present, so in animal substances it is rarely absent,—indeed, an essential chemical character of animal matter is, that it yields *ammonia* when heated, and hence the well-known pungent smell of burned hair, feathers, &c.; hence, also, the manufacture of carbonate of ammonia, and other ammoniacal compounds, by the destructive distillation of horn, hoof, the refuse of bone, and ivory turnings, and other waste animal matters which are collected for the purpose; hence, too, the terms *salt* and *spirits of hartshorn*, applied to the solid and liquid carbonates of ammonia, derived formerly from hartshorn shavings, but now, indiscriminately, from any kind of bone.

There are two other elementary substances of common occurrence in animal bodies, namely *sulphur* and *phosphorus;* these confer other peculiarities upon the proximate principles containing them, especially distinguishable in the products of putrefaction; animal matters having altogether a greater tendency than vegetables to run into that kind of complicated and variable decomposition called *putrefaction*, and to exhale, under such circumstances, peculiarly nauseous odours, dependent, in many instances, upon the exhalation of compounds of phosphorus and sulphur.

Animal fat is so far peculiar, that, when purified by fusion, and separated from cellular membrane, it contains no nitrogen, and no water, —in fact, animal and vegetable oils are analogous, if not identical in composition, and are (in reference to ultimate composition,) triple combinations of carbon, oxygen, and hydrogen.

The other proximate principles of animals, and among these *muscular fibre*, are commonly associated with a large quantity of *water*, so that they lose greatly in weight by the mere operation of drying: to this water their tendency to putrefaction is, in great measure, to be ascribed; for if animal matter be cautiously dried, it remains comparatively unchanged. The average quantity of water contained in the fleshy fibre may be regarded as amounting to about 75 *per cent.*, so that the real nutritive matter of a pound of beef, for instance, is only four ounces; and the flesh of fowl, and even of fish, is very nearly similarly composed. Hence, the high relative nutritive power of all kinds of dried meat, and, to a certain extent, of salted meat, smoked meat, &c.

It is the absence of this component moisture, or elementary water, *as it may* be called, in hair, horn, feathers, dry bone, &c., which renders *those products* so permanent and durable, as compared with skin, membrane, and muscular flesh, which, as is well known, are very prone to putrefactive changes. But a state of greater or less humidity seems essential to *living matter;* so that, as far as the soft solids are concerned, a living animal may be regarded as a mass of organized matter, softened by the presence of water to the amount of about three-fourths of its weight; and *drying* always kills the part, for vitality is not existent in dry matter.

There are only three proximate principles of animals which necessarily come before us as articles of food, though they are variously modified in different parts of the animal; these are, *fat*, *gelatin*, and *albumen;* so that, in this point of view, the dietetic history of animal substances is less complicated than that of vegetables. These three proximate principles are all to be found in the muscular, or fleshy parts, and may be separated by simmering in water; the *fat* separates and floats upon the surface of the broth, from which it may be skimmed off, or allowed to cool and concrete; the *gelatin* is dissolved, and, if in sufficient quantity, it confers the property of *gelatinisation* upon the broth, when cold; the insoluble residue is the *albumen*, or fibrin.

The general properties of fat, such as its fusibility, greasiness, and combustibility, are well known, and importantly applicable to numerous purposes of the arts; and the products of its combustion show that, like vegetable oil, it is a compound of carbon, hydrogen, and oxygen; and when carefully purified, so as to free it from all adhering cellular membrane, it contains no nitrogen, and is resolved, by combustion in excess of oxygen, into carbonic acid and water only[*].

When, in the process of simmering, or stewing, (that is, digesting meat in water at a temperature considerably below the boiling point,) the whole of the fat has been separated, and removed from the surface upon which it floats, we find one of the other proximate principles remaining dissolved in the liquor or broth, which, as it confers upon it the property of gelatinising when cold, has been called *gelatin*. It is a very common and abundant animal product, but chemists differ in opinion as to the state in which it exists in the original substance from which it is extracted by hot water—namely, whether it is ready-formed and merely dissolved by the water, or whether it is essentially modified, or even *formed*, by the joint action of heat and water upon some peculiar principle,—whether, in fact, it is to be considered as an *educt* or a *product*. In reference, however, merely to the object of this paper, the question is of little importance; suffice it to say, that when certain animal substances are digested in hot water, a peculiar principle is obtained, which yields a more or less dense tremulous mass, or *jelly*, on cooling. It is afforded by tendons, ligaments, cartilages, but especially by certain membranes, and by the skin: horn, and bone, when shaved or rasped, and duly boiled in water, also yield it, as is seen in

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[*] The ultimate elements of *mutton-suet* may be referred to as illustrating the general composition of animal fat. When purified by fusion in hot water, and gentle pressure, so as to separate the membrane or cellular structure in which it is enveloped, it consists of,—

Carbon . . .	790
Hydrogen . .	118
Oxygen . . .	92
	1000

hartshorn-jelly, and in glue, portable soup, and other forms of it, chiefly obtained by boiling bones, clippings of skin, and various other offal; *isinglass*, which is the dried sounds, or air-bladders, of certain species of sturgeon, is one of the purest forms of gelatine, and may be referred to as presenting us with all its leading characters. When soaked in warm water, it swells and dissolves at a temperature below 150°; its solution gelatinises when cold, unless it be very dilute, and in that case the presence of gelatin is easily manifested in it, by the addition of a few drops of tincture of galls (or of any other astringent vegetable infusion), which throws down an insoluble white precipitate. Sulphate of platinum is also an excellent test for gelatin; it forms a gray or brown precipitate in extremely dilute solutions[*].

The absolute *nutritive powers* of fat, and even of gelatin, but especially of the former, are somewhat problematical. If one dog be fed upon broth, and another upon the residuary meat, the latter will thrive, and the former dwindle; but such a comparison, to lead to a fair inference, should be made upon equal weights of dry gelatin and dry albumen.

The third nutritive proximate principle of animal food is *albumen;* there are many modifications of it, and, as obtained in our present experiment, merely by digesting muscular flesh in hot water, it remains in the form of an insoluble fibrous residue, and is sometimes termed *fibrin*[†].

Albumen, in this form, is insoluble in water, and when carefully dried it shrinks, and again softens when digested in warm water.

But there is another most important state of albumen in animal bodies—namely, the *liquid state:* as such, it forms a characteristic ingredient in the *blood;* but we cannot take a better example of liquid, or uncoagulated albumen, than the *white of egg*, from which, indeed, the term *albumen* is derived, and which presents it in all its characteristic properties. In this liquid state its solubility in, or miscibility with, water, deserves especial notice; but it differs from gelatin in being solidified or coagulated by heat, by many acids, by alcohol, and by several other agents.

Its coagulation by heat is well known, and illustrated by boiling an egg, but we are ignorant of the cause of this change of form which it suffers at a certain temperature. Its coagulation by alcohol and by acids is equally inexplicable. As liquid albumen is sensibly alkaline to

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[*] *Leather* is the insoluble compound of vegetable astringent matter (or *tannin*,) and gelatin, above adverted to; skin is a condensed form of gelatin, and by soaking it, after due preparation, in a pit containing oak-bark and water, the tannin of the bark combines with the gelatin of the skin. The *ultimate composition* of gelatin is as follows:—

Carbon . . .	483
Hydrogen . .	80
Oxygen . . .	276
Nitrogen . .	161
	1000

[†] The following are the *ultimate elements* of albumen:—

Carbon	516
Hydrogen	75
Oxygen	259
Nitrogen	150
	1000

delicate tests, it has been supposed that the abstraction of alkali is essential to its coagulation, and that this abstraction is effected by alcohol and by acids, and, in some less evident manner, by heat; but all acids do not coagulate albumen,—the acetic acid, for instance, or vinegar, does not even render it turbid: it is possible, however, that this may depend upon the solubility of the acetate of albumen which is formed, whilst the sulphate, nitrate, and muriate are, to a greater or less extent, insoluble.

But when albumen is in very dilute solution, or in certain states of combination, the above-mentioned tests of its presence become indistinct, and often ineffective; in these cases we detect it, either by a solution of corrosive sublimate, which produces a white cloud in solutions or liquids containing the minutest trace of this proximate principle, whilst gelatin is not affected by it; or by the addition of solution of ferrocyanuret of potassium; but this latter test requires that a few drops of acetic acid should have been previously added to the liquid under examination, and then the presence of albumen is, as in the former case, announced by a more or less dense white precipitate. Albuminous solutions, like those of gelatin, are rendered turbid by tincture of galls, but the precipitate is much less distinct and insoluble; muscular fibre, however, and other albuminous structures, may be, to a certain extent, tanned by vegetable astringents.

So far, then, we have no difficulty in recognising fat, gelatin, and albumen; and in the analysis of the flesh of animals, we find these three proximate principles, together with a considerable quantity of *water*, the relative proportion of which may be judged of by careful drying, (at a temperature not exceeding 212°,) during which the fibre shrinks and loses weight by its evaporation. Having dried the animal fibre, and so ascertained the relative proportion of its component *water*, the *fat* and *gelatin* are then separated by the action of warm water, and the insoluble residue is *fibrin*, or albumen. In making this analysis, the fibre, before drying, should be cut into shreds, and these, when dried, should be further grated, in order to ensure the solvent action of the water; for if a piece of flesh be *boiled* in water, the exterior becomes hardened and coagulated, and the interior portions are thus protected from the solvent power of the water, the meat is rendered hard, and the broth is poor; but if *simmered* (or *stewed*, as it is usually called,) at a temperature of 140° or 150°, the albumen or fibrin then remains soft and pervious, and all the soluble matters of the flesh are extracted, whilst the residue is tender and easily digestible. These facts are well known in practice, though the *theory* of these, and other culinary operations, has not hitherto found its way into our cookery books.

During the operations of which we have spoken, whether conducted in the kitchen or in the laboratory, we get evidence of another proximate principle, small in quantity, but peculiar in character, which chemists, by way of distinction, have called *osmazome**; it is sometimes termed *animal extractive*: it is brown, soluble, and deliquescent, and has a very characteristic and (to hungry persons) agreeable smell,—it is, in fact, the source of the peculiar fleshy odour of boiled meat, broth, and soup.

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* *From the Greek words* οσμη, *osme*, odour, and ζωμος, *zomos*, broth.

It is modified, and often apparently produced, in the process of roasting, when it is more or less decomposed or burned, becoming browner and more odorous, and giving colour and flavour to the gravy: it dissolves in water and in alcohol, and does not gelatinise: hence is distinct from fat, from albumen, and from gelatin; in most cases it is, however, apparently a *product*, that is, it did not pre-exist in the fibre, but is the result of the action of heat upon one or more of the original principles.

We have now enumerated and described the principal proximate components of the soft and nutritive parts of animals, and especially of muscular fibre; but we always find them associated with various saline and earthy bodies, and they very commonly afford traces of phosphorus and sulphur. Thus, when we burn a piece of muscular fibre, it leaves a charcoal, which is very difficultly burned away to an ash, and which, when digested in muriatic acid, yields phosphate of lime, and some other phosphoric salts. The yolks of eggs, and the brain of animals, also contain a notable proportion of phosphorus, in combination with a peculiar fatty matter, and a modification of albumen; and it has been asserted by Couerbe, that the proportion of phosphorus in the brain of persons of sound intellect amounts to from to 2 to 2½ *per cent.*, whilst, in idiots it always falls short of, and in violent maniacs exceeds, that healthy standard,—a statement which deserves severe scrutiny, and which, if correct, may lead to some very important physiological and practical results.

Sulphur is also found in several varieties of animal food; it is the cause of the discoloration of silver spoons by boiled eggs; sulphuretted hydrogen is also evolved by putrid eggs.

Enough has now been said to give the reader a general notion of the chemical nature of our ordinary animal food: it is less abundant in distinct proximate principles than vegetable food; yet the essential nutritive powers of both are referable to three or four distinct principles only.

There are, however, exclusive of the soft solids of animals, some other products which, on account of their dietetic importance, may require a few additional remarks. One of these is *bone;* and if we look at this substance chemically, we shall find in it some remarkable peculiarities. Bone may be considered as a combination of a hard or earthy part, with a soft, or animal part; the latter is easily destroyed by burning, and then a white, porous, earthy-looking residue is obtained, which the old chemists called *bone-earth*, but which consists chiefly of phosphate of lime, with some carbonate of lime, and traces of sulphate and fluate of lime, and of magnesian salts*. In bone, then, we have a vast magazine, as it were, of that extraordinary elementary substance,

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* 1000 parts of the dry bones of the ox contain—

Animal matter	510
Phosphate of lime	377
Carbonate of lime	100
Phosphate of magnesia . .	13
	1000

phosphorus; every pound of bone-ash, containing about twelve ounces of phosphate of lime, about half of which is phosphoric acid*, and of phosphoric acid, somewhat less than one-half is phosphorus. In fact, all our supply of phosphorus is at present exclusively derived from bone.

But about half the weight of bone consists of *animal matter*, of which a small portion is fat, and the remainder a peculiar form of gelatin, together with some albumen: this is commonly termed the *cartilaginous* portion of the bone; it does not dissolve in the cold dilute acids; and accordingly, if a bone be steeped in dilute muriatic acid, the whole of the earthy salts will be dissolved, and the animal portion remains in the form of a soft flexible skeleton, as it were, which, by long-continued boiling in water, is almost entirely soluble, and yields a broth or soup which jellies on cooling.

It is, however, impossible to extract the gelatin of bone by the mere action of boiling water, even when the bone is ground or rasped, for the earthy matter with which it is combined protects it from such solvent power; much of it may, however, be obtained by subjecting the comminuted bone to the action of water a few degrees above the boiling point, as in Papin's "Digester;" but at that temperature the jelly is itself apt to be altered; hence the great advantage of the previous use of dilute muriatic acid, by which the phosphate of lime is dissolved, and the remaining gelatin is then soluble when duly simmered, or stewed with water. It is well known that dogs will not only live, but even fatten, on bones; they gnaw them into small fragments, and upon these the gastric juice acts in such a way as to abstract the gelatinous or nutritive part, whilst the earthy portion is rejected as excrementitious.

But there is an extraordinary circumstance respecting the gelatin, or albumino-gelatinous portion of bone, which is the manner in which it resists putrefaction, even under the influence of air, and such agents as would speedily produce its decomposition, were it not combined with phosphate of lime. Bones which are a hundred years' old are as nutritive as those which are recent; except, indeed, they have been exposed to certain rarely-occurring agencies. Bones found in ancient tumuli, and even fossil bones as they are called, those, namely, of ancient and antediluvian animals, have been found to retain their animal or gelatinous portion, and to furnish broths and jellies like those of recent bones, especially where they have been sealed up in stalagmite, and protected from the continuous action of air and water.

It will be obvious from the preceding statement that we are in the habit of wasting an immense quantity of available animal food in the form of *bone*. Bone constitutes upon an average a fifth part of the weight of an animal, and one-third of the weight of bone may be reckoned as good substantial food. The mean weight of an ox is about 660 lbs. The bones, therefore, may be estimated at about 132 lbs.; and this weight of bones contains about 40 lbs. of gelatin; not in the form of soup

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* Phosphoric acid consists of—

Phosphorus	444
Oxygen	556
	1000

or even in its jellied or tremulous form, (that is, combined with water,) but in its *dry* state. The weight of butchers'-meat consumed in London annually, is supposed to be about 172 million of lbs., = 35 million lbs. of bones, = 11 million lbs. of dry gelatin, or real nutritive matter, which is so far *wasted* as not to be applied to the support of human life *. The bones of pork, game, poultry, and fish, not included in the above notice, must also be of great amount; and to these, in reference to *nutritive matter*, those of animals not usually eaten, such as horses, cats, dogs, &c., might, strictly speaking, be added. From all, or any of these, an excellent dry gelatin, or portable soup, might be prepared (and sold for about 2*s.* per lb.), equivalent to three or four times its weight of raw meat.

In times of scarcity these are matters of real importance, and as the animal matter of bone is effectively embalmed and preserved from all ordinary decay by its association with the earthy salts, magazines of bone might be stored up for besieged towns, or other occasions of dearth or famine †. In short, the whole subject is interesting and important, and an honorary reward for the best essay on the "Cookery of Bone" would not be ill bestowed: soups innumerable, and other palatable and nutritious dishes, might spring out of such an inquiry, especially if pursued by any good cook who would condescend to learn a little chemistry.

THERE is another article of animal diet which, on account of its highly important uses and remarkable composition, has always excited much of the chemist's attention, namely, MILK ‡. It partakes, to a singular extent, of the properties of vegetable as well as of animal food; for whilst, on the one hand, we have in it a matter closely resembling albumen, namely, *curd*,—that is, a perfectly animalized and nitrogeniferous principle,—there is also a substance intermediate in its chemical properties between gum and sugar, which has been called *sugar of milk;* and lastly, *butter*, which is very analogous in composition to the fixed oils: water, and a little acid and saline matter, are the remaining constituents of milk.

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* The various uses which are made of bone in reference to articles of turnery, and the products of its distillation and combustion, such as various ammoniacal compounds, ivory black, animal charcoal, &c., are not noticed here, as they have not direct reference to our present subject. Ground bone also forms an excellent manure; and when boiled, the fat or grease, which separates in skimming, is used in the manufacture of soap. A portion of bone is also used in the manufacture of *prussiate of potash*, a salt largely consumed by dyers and calico-printers, and in the production of Prussian blue.

† See, in reference to this subject, two papers by Gimbernat in the Quarterly Journal for 1827.

‡ Skimmed milk contains:—

Curd . . .	28	. 28
Sugar . . .	35	Whey 972
Lactic acid and salts	8	
Water . . .	929	
	1000	1000

Cream is composed of,—

Butter . . .	45
Curd . . .	35
Whey . . .	920
	1000

The separation of *cream* from milk is one step towards its analysis; and this, by *churning*, is resolved into butter and butter-milk; the butter-milk is little else than sourish whey, for during churning some oxygen is absorbed, and a little lactic acid produced: the butter is originally diffused through the milk in the manner of an emulsion, and its particles aggregate, to the exclusion of the watery part, during churning.

If dilute sulphuric acid be added to milk, the curd, or *caseum*, separates in combination with the acid, which may be afterwards abstracted by chalk, and the separated caseum then dissolved in water. But the best coagulant of milk, for the purpose of separating the caseum, is *rennet*, which is the internal membrane of the calf's stomach, (that of other animals equally answering the purpose,) as in the common operation of making curds and whey. If the curd be now washed and dried, it has many properties in common with albumen, and, like that principle, abounds in nitrogen [*]; whilst the gum, or sugar of milk, is destitute of nitrogen, and resembles vegetable gum, starch, or sugar, in its ultimate composition [†].

Among vegetable products, wheat-flour is pre-eminently nutritive, and in it we have *gluten*, which corresponds to animal matter, and is nitrogeniferous, and *starch*, which is destitute of nitrogen, and resembles gum and sugar. Now starch, with more or less gum and sugar, are the nutritive principles of our common esculent vegetables; but these require the addition of animal food, that is, of a nitrogeniferous principle, which in milk and in wheaten bread is already provided by nature. The cause of this necessity of mixed food for man, for the carnivorous tribes, and indeed generally for the higher orders of animals, requires to be more fully examined into, and will be touched upon in our next, and concluding paper [‡].

[*] The ultimate elements of curd are,—

Carbon	608
Hydrogen	72
Oxygen	116
Nitrogen	204
	1000

[†] The ultimate elements of crystallized sugar of milk are,—

Carbon	400
Hydrogen	67
Oxygen	533
	1000

[‡] The chemistry of *cheese* ought strictly to be taken up here, but any useful details in regard to it, would require more space than is at present to spare.

A BRIEF SKETCH OF ENGLISH SCIENTIFIC LITERATURE,

FROM THE INVENTION OF PRINTING TO THE END OF THE SIXTEENTH CENTURY.

The progress of science in this country, from the period in which Caxton erected his press in Westminster, to a century and a-half posterior to that event, has hitherto almost entirely escaped the pen of mathematical historians, both English and foreign. It is not easy to account for this negligence on the part of our own writers; for, as the following portion of this article will show, it has not been for any deficiency of material; while, on the other hand, our Continental neighbours being, in most cases, unable to obtain inspection of the original works, have often misrepresented their merits. Under such circumstances, therefore, a sketch of the progress of English scientific literature during the above-mentioned period cannot but be acceptable; and if inadequate descriptions of some works have anywhere been given, the reader will more readily excuse it, if he takes into consideration the extreme difficulty of procuring the treatises referred to.

In 1481, Caxton printed,

> *The Mirrour of the World; or, Thymage of the same.* fol. 100 leaves.

This work appears to be a general miscellany of arts and sciences as practised at that time; witness the contents of a few of its chapters:—

Book 3.	Chap. 5, 6.	How the eclypses of the sunne and moone come.
——	Chap. 9.	Wherefore and why the world was measured.
——	Chap. 17.	How moche the erthe hath of heyght, how moche in circuyte, and how thicke in the myddle.

In the *Werk of Sapience*, also, which was both written and printed by Caxton, are stanzas on Arithmetic, Geometry, and Astronomy.

The great number of romances that issued from the presses of the early English printers may be regarded as a criterion of the then existing literary taste; with the exception of the above-mentioned work, Caxton printed nothing which could be classed under any division of general science. Forty years afterwards, Richard Pynson had the honour of printing the first English work on arithmetic, which was also the first that treated exclusively of any portion of the mathematics: it must be remembered that, at this time, Cardan and Tartalea were employed on cubic equations.

> *De Arte Supputandi, Libri Quatuor, Cuthberti Tonstalli.* 4to. London, 1522, and again at Paris.

The writer was successively bishop of London and Durham; his work on arithmetic passed through several editions, and, in point of simplicity, well deserved its popularity, but it contains examples only of the simplest kind.

It may not be wholly irrelevant here, to make a few observations on the introduction of Arabic numerals into England. A passage in

Chaucer's *Dream* has been supposed by some to imply that they came into use shortly before that author's time:—

> Shortly it was so full of bestes,
> That though Argus the noble countour
> Y state to rekin in his countour,
> And rekin with his figures ten,
> For by the figures newe all ken.

But though they might have been introduced even some time before Chaucer wrote, yet it is very evident that they were by no means admitted into such universal employment as the above quotation would appear to intimate. Ashe says that they appear in Bacon's *Calendar*, written about 1292, and Vossius, at farthest, before 1250. As late as 1505, in the churchwarden's accounts at Lambeth, the Roman numerals are used, and no trace of the "figures newe" appear; the like is also found to be the case in the other parishes. They were, however, generally employed in mathematical manuscripts as early as the fourteenth century, but it was most probably not till the middle of the sixteenth that they began to be universally adopted.

It was some time before another work was produced of even equal pretensions with that by Tonstal. In 1537, John Hertford printed at St. Albans,

> *An Introduction for to lerne to recken wyth the Pen and wyth the Counters, after the true cast of Arismetyke, or Awgrim, in hole Numbers, and also in broken.*

This book I have not seen, but, from the near resemblance of their titles, should suppose that it was the same with another printed by Nycolas Bourman, in 1539:—

> *An Introduction for to lerne to recken with the Pen, or with the Counters, accordynge to the trewe cast of Algorisme, in hole Numbers, or in broken, newely corrected. And certayne notable and goodlye Rules of false posytions thereunto added, not before sene in oure Englyshe tonge, by the whiche all maner of diffycile questions may easily be dissolved and assoylzed.* 8vo. 110 leaves.

It treats of the common rules of arithmetic, and a similarity may be traced in some parts to Record's "Ground of Arts." The latter portion of it contains the "Introduction for to lerne to recken with the Counters, with divers rules belongynge to the same," intended for those who cannot read or write, or "such as have not at some time their pens and tables ready with them;" these counters continued in use among the ignorant for some time afterwards, and I accordingly find the clown, in Shakspeare's *Winter's Tale*, confessing his inability to reckon without them.

Arithmetic is the only part of science on which any efforts had yet been made. It was not till 1560, or thereabouts, that Richard de Benese wrote his book on *Surveying*, which was several times reprinted. This is the first English work of its kind, and contains only the simplest rules. In 1577, Valentine Leigh published an extension of the work of Benese, entitled "The most profitable Science of Surveying," which was reprinted as late as 1596; of Digges' Tectonicon I shall presently speak.

With respect to astronomy, little was done in England during the first half of the sixteenth century, and the few treatises that appeared on that subject were mixed up with astrology. In 1543, the great work of Copernicus appeared; and I will now proceed to show how his opinions were received in England. From a communication of the Rev. J. Hunter to the Royal Astronomical Society, it appears that the first English work that openly avowed his system, was not published till fourteen years afterwards, under the following title:—

> *Ephemeris Anni* 1557 *currentis juxta Copernici et Reinhaldi Canonis fideliter per Johannem Feild Anglum, Supputata ac examinata ad meridianum Londinensem qui occidentalior esse indicatur à Reinhaldo quam sit Regii Montis, per horam* 1 *kr.* 50. 4to. Londini, 1556. Septembris 12.

In the course of which Feild avows his conviction of the Copernican system; as also Dee, in his preface which is subjoined to Feild's work. Thomas Digges, in 1573, and John Blagrave, in 1596, as I shall afterwards show, declared themselves Copernicans. These, with Robert Record, are the only ones that I can point out in this century, as having received his system.

William Bourne, or Boorne, who was elected to the office of portreeve, or chief magistrate, at Gravesend, in the year 1571, and was one of the proprietors of the Gravesend barge, appears to have possessed an active mind, with a proficiency in the mechanical arts as known at that time. The following is a list of all the works that I have seen by him:—

1. *An Almanacke and Prognostication for three years,—that is to saye, for the yeares of our Lord* 1571, 1572, *and* 1573. 12mo. 1571. Printed by Purfoot.
2. *A Regiment for the Sea; conteyning most profitable Rules, Mathematical Experiences and perfect knowledge of Navigation, for all Coastes and Countreys; most needful and necessary for al Sea-faryng men.* 4to. Thomas Dawson, 1577. Reprinted 1592, 1611, 1620.

Containing common-place matters relating to navigation, and concludes thus:—

Welcome the wight,
That bringeth the light.

3. *Inventions or Devises, very necessary for all Generalls and Captaines, or Leaders of Men, as well by Sea as by Land.* 4to. Thomas Woodcocke, 1577.

In this curious little treatise he mentions a method of propelling boats by means of external wheels. It occurs among the devices, of which he says, "Some of them I have gathered by one meane, and some by another, but the most part of them hath been my owne invention, upon sundry causes in the affaires I have had to do." At the close of the work he treats on steelyards, which he remarks were introduced from Spain, telegraphs, and telescopes.

4. *Treasure for Travaillers by Sea or Lande.* 4to. Thomas Woodcocke, 1578.

A curious work on navigation, mensuration, mechanics, and the natural causes of several phenomena.

5. *Almanacke for Ten Yeares, beginning at the Yeare* 1581, *with certaine necessarie Rules.* 8vo. R. Watkins and James Robertes, 1581.
6. *Art of Shooting in great Ordinance, contayning necessarie matter for all sorts of servitours, eyther by Sea or Lande.* 4to. Thomas Woodcocke, 1587.

Blundeville cites another work by Bourne, entitled the *Attractive*, but I have not seen it.

Leonard Digges and his son Thomas contributed, perhaps more than any others, to the advancement of English science during the sixteenth century. I have diligently searched throughout their works for information relative to the system that the former believed,—Ptolemaic, or Copernican,—but was unsuccessful; his son, however, who published a treatise, entitled *Alæ sive scalæ Mathematicæ*, (4to. 1573,) professes in it his conviction of the truth of the Copernican system, and to his editions of his father's work entitled *Prognostication Everlasting*, (fol. 1555,) published in 1578, and again in 1592, he made the following addition:—

> *A perfite Description of the Celestiall Orbes according to the most auncient Doctrine of the Pythagoreans, lately revyved by Copernicus, and by Geometricall Demonstrations approved.*

In the preface to this addition, he openly expresses his disapproval of the Ptolemaic, and belief in the Copernican system*.

Many years afterwards (1634) a posthumous work of Thomas Digges was published, entitled, *Nova Corpora Regularia; seu, quinque Corporum Regularium Simplicium, in quinque alia Regularia Composita, Metamorphosis*, with some problems added by the editor. Hutton slightly mentions this work, but appears not to have seen it; a copy is in the library of the Royal Society.

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* I must here point out a singular error committed by the writer of an article in the "Companion to the British Almanac for 1837," entitled *English Mathematical and Astronomical Writers.* He mentions that T. Digges republished his father's work in 1592, (being apparently unacquainted with the earlier reprint,) accompanied by "a perfect description of the celestial orbs, according to the most ancient doctrine of the Pythagoreans," and he adds, "their *Astrological* doctrines, we presume, not their reputed *Copernican* ones." It is rather extraordinary that he should have made that mistake, even though he had not seen the book itself, for Wood expressly affirms "cui subnectitur orbium Copernicanorum accurata descriptio;" and immediately he proceeds to prove Digges to be a Copernican, from passages in his "Alæ, &c.," written five years before the first, and nineteen years before the second, reprint of the treatise by the elder Digges.

The *Tectonicon* of L. Digges, a small treatise on mensuration, first published in 1556, passed through a great number of editions; and the *Pantometria* (fol. 1571 and 1591) was another work on the same subject, but rather more extensive.

In 1557, Dionis Gray published his *Storehouse of Brevitie in Woorkes of Arithmetick*, which was reprinted in 1586 (8vo.), and, considering the period in which it was written, deserves praise. Five years afterwards Humphrey Baker, citizen of London, set forth his *Well Sprynge of Sciences, which teacheth the perfect Worke and Practice of Arithmetike, &c.*, treating principally of commercial arithmetic: and this, perhaps, was the cause of its extensive circulation,—it continued to be reprinted as late as late as 1687. In 1588, John Windet printed *The Peice of Book-Keeping and Arithmetick*, by John Mellis; and, in 1582, Thomas Masterston published the first and second books of his arithmetic, "Verie necessarie for all men. Nothing without labour, all things with reason." An addition to the first book appeared in 1594, and the third, in 1595. About the same time the arithmetic of Peter Ramus was translated (4to. 1592), as also the work of Ustitius on the same subject (12mo. Basil, 1579), by Thomas Hood (8vo. 1596).

Richard Eden, in 1561, translated from the Spanish a work on navigation by Martin Cortes, (Cadiz, 1556,) which treats of as much of the subject as was then known. He shows the error of the plane chart, which was defended by Mendina, a countryman of his, in a treatise first published in 1545, and afterwards translated into English (4to. 1595.) The former work appears to have been the favourite in England, and the latter on the continent; Cortes's work passed through three editions. In 1577, the celebrated John Dee published his *Brytish Monarchie, or Memorials pertayning to Navigation*, of which only one hundred copies were printed; I have been informed that it is a very excellent work, and the best by that writer, but have not seen it. A celebrated bibliographer styles it "the rarest work in English literature." The works on navigation by Bourne have already been noticed; Captain John Davis, in 1595, published his *Seaman's Secrets*, an excellent work, according to Hutton, who gives an erroneous date to it; in 1597, William Barlowe put forth his *Navigator's Supply*, containing several curious matters relating to navigation, which will even now be read with interest. The last works that appeared in this country relative to this subject, were two by the celebrated Edward Wright, entitled, "Certaine Errors in Navigation corrected," and "The Haven-finding Art," both of which have been so fully described in other works, that any further notice of them here is unnecessary.

The works of John Blagrave, of Reading, contain a curious mixture of good and bad. In 1596 he published a description of a new astrolabe adapted to the principle of Copernicus's system, but yet mixed up a little with astrology.

> *A necessary and pleasaunt Solace and Recreation for Navigators in their long Journeying, containing the use of an instrument or generall Astrolabe: newly for them devised by the Author, to bring them skilfully* acquainted with all the Planets, Starres,

and Constellacions of the Heavens; and their courses, movings, and appearances, called the Uranicall-Astrolabe.
In which, agreeable to the Hipothesis of Nicolaus Copernicus, the starry firmament is appointed perpetually fixed, and the earth and his horizons continually moving from West towards the East once about every twenty-four houres.
Fraught also by a new devise with all such necessary supplements for Judiciall Astrology, as Alkabitius and Claudius Dariottus have delivered by their tables. 4to. Thomas Purfoot, 1596. 33 leaves. Dedicated to Lord Charles Howard.

Several works still remain undescribed, but the limits of this article will not allow of their mention. I have purposely omitted saying much of the works of Record, and a few more, because their merits have been completely canvassed by other and more able writers.

J. L.

THE TELFORD-MEDAL.

In the Autumn of 1834, Mr. Telford, the eminent engineer, left, in the most unrestricted manner, the interest of two thousand pounds, to be expended in annual premiums under the direction of the Council of the Institution of Civil Engineers. In February, 1835, the Council determined that part of this noble donation should be distributed in the shape of medals; and we believe that Mr. Wyon, the celebrated medallist, of the Mint, either at that time or very soon afterwards, received an order for the preparation of a medal for the purpose. To whom the *design* was confided we never heard; but a "suitable device" it was resolved should be had. A medal, we know, is not like a mushroom—the product of a few hours, nor yet should it resemble the flower of the American aloe, and require a century for its developement. Mr. Wyon took rather more than two years to execute the order, and in the month of May last the medal was delivered by him to the council. For the sake of a small *coup d'etat*, the successful but unrewarded candidates, though they had waited two sessions, were not apprized of this important event, but such of them as happened to be at a dinner at the London Coffee House on the 3rd of the last month, were surprised by a request to walk up to the chairman and receive the guerdon so long looked for*.

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* It appears to us that this was a very improper time and place for such a ceremony. Every member and associate of the Institution had a right to have had notice, and to be present at it, without being obliged to pay 20s. in addition to a heavy yearly subscription. To *select* such a scene for the first distribution, marks an imbecility of judgment somewhere that ought to be looked after. The occasion was an epoch in the life of the Institution which can never return. It presented a peculiar opportunity of exciting the rising members of the profession, by referring to those who have passed away, and to the mighty works that were done in their days,—to the widening and extending prospects which are opening every year to the profession,—and of urging the necessity of temperance, of application, of unceasing inquiry, and of never-ending experiment, to the student of engineering. What a contrast was the tavern-revel! It is more than probable that the students who could not be present will be the men who will one day maintain the pre-eminence of British engineering. It is quite certain that those who were, had no estimable example held out to them by some who hold nominal rank in the profession.

At that hour, criticism could not be expected to see very distinctly, or be disposed to say anything that was impolite, however true. One symptom only, which could be depended upon as a demonstration of the opinions of those who saw the medal, was observed—*the eagerness of every one was allayed by the slightest glance!* There appeared nothing on either face to rivet the eye of the observer,—to present a new idea, or excite an agreeable sentiment. It passed around the company as quick as the light of the little brand in "Jack's alive!"

That the medal under consideration has more than usual claims to notice might be easily proved, if required. It would only be necessary to mention the names of the artist, of the founder, of the trustees, of the profession whose improvement it is intended to facilitate,—all, or indeed any, of these, would be reason sufficient to excite curiosity and justify examination. We propose to describe the medal, and then offer such remarks as its inspection may suggest.

The obverse of the medal has a head in profile of a person about the middle age, the neck and shoulder naked; in a vertical line in the rear of the head is a single word, thus:—

TELFORD.

Under the shoulder we find, W. WYON, A.R.A.

On the reverse is the usual view of the Menai Bridge, and beneath it the same "tall admiral" we have always seen there, her top-gallant masts set, her pennons flying, and every sail bent; but there is here, in addition, a steamer, apparently an ambitious rival, working against a head-wind in order to obtain the same enviable station. It is plain, from the volume of smoke issuing from her chimney, that her *engineer* has been "firing" in his peculiar manner, either to salute the platform over head, or, perhaps, in recognition of his brethren of the same name snugly *incorporated* beneath the bank below. On the left of the Strait a large lump of the Bangor shore projects across the base of the scene, and forms the fore-ground to the picture. Upon it is some rock and tree-work; a specimen of fern of a gigantic species; a male and female native; a rubble wall, seen not only in elevation but in *section;* the rear of an empty cart, and that of a left-handed driver, going down a deeply-rutted lane. On the right is a strip of Anglesea, another lane and two cottages, but not the "leg*" of the gallant Marquis on the hill in the distance. The water has, besides the larger vessels, a few waves and fishing-boats on its surface; and there are some clouds in the sky, so well defined, that our meteorologist at once pronounced them to be *cumulo-stratus!*

Upon the exergue, under all this collection of wonders in nature and art, stands, not a description, as usual, of the subject alone, or at least some circumstance connected with it, but this inscription in capital letters:—

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* The name given by the natives to the column erected on a height in the *neighbourhood* of the Menai Bridge, by the Principality in honour of the Marquis of *Anglesea,* who lost a leg at Waterloo.

INSTIT: CIV: ENGINEERS.
INCORP: 1828.

The above is a faithful detail of the work of art before us. Is it what it ought to be? Let us first compare it with the one originally intended and described by *the Special Meeting*, already referred to, of the Council of the Institution of Civil Engineers, held the 23rd February, 1835[*]. That meeting determined that the honorary medals should have "a head of the late president on one side, *surrounded by the words 'Institution of Civil Engineers, founded* 1818;' and on the other, '*Telford Medal,' and a suitable device, leaving a space for the name of the successful candidate, and the object of the reward.*" Little, we may with truth say *none*, of this is to be found on the medal just described. But a special pleader may say, the medal under consideration is one of "such other description of honorary medals as shall be determined by the Council." If this should be said, we shall venture to doubt it; in fact, we should have little hesitation in denying it; for though we may think the Council has been strangely tardy in the discharge of this part of a sacred and important duty undertaken by them, we believe their common sense would have instantly rejected such a design for an honorary medal, if it had been submitted to them before the execution of the die. Who, then, has disobeyed the sensible (though not altogether perfect) instructions of the Council of February, 1835? and under whose direction has it happened that not a single feature of a rational reward-medal has been preserved?

If the remarks which we may make upon this medal be tinctured with severity, it will be produced by the admiration we feel for the medallic art, by the high estimation in which we hold the products of the long and precarious labours of the medallist, by the conviction that medals are monuments, and ought to be faithful, characteristic, complete; that in them Genius may not only be eloquent, as in sculpture,—rhetorical, as in painting,—but, further, may even be epigrammatical, and witty in an exquisite degree. We think invention and composition in medal-designing have nearly as wide a range as in poetry, and can run through all the various and innumerable quotations which lie between the expression of a sublime idea and that of a broad pun. Finally, as we are conscious that medals may become historical evidences of the intellect and skill of the period in which they were designed and struck, we are jealous for the reputation of our time, and we may add, with great sincerity, for that of the Institution whose existence is now attempted to be recorded by the medallic art.

We fear that the design upon the medal recently produced by Mr. Wyon for the "Telford-medal" ought not to satisfy any party, neither the personal and professional friends of the late president, nor the Institution of Civil Engineers, nor the meritorious individuals to whom in future it may be decreed as a distinction, nor to the patrons and lovers of native merit, nor to men of taste in general.

Almost every personal friend of the late Mr. Telford accuses the inscription on the obverse, brief as it is, of falsehood! "It is not Telford," has been the general exclamation. Sufficient accredited memorials remain of what Mr. Telford was, to justify the negation. This is a melancholy deficiency, and whether the physionomy or the phrenology on the medal be better or worse, it has the demerit of imposing upon posterity. Again, the affected brevity of the inscription is unfitted to the subject of the medal. Such names as BYRON, GOETHE, SCOTT, may bear this denudation of all date and circumstance, without fear of obscurity; but we think that (out of the class of princes and warriors) there is not another name that has passed away in the present age, that would. Extensive as are the public works of the late Mr. Telford, bearing, as some of them do, characters the fittest to excite admiration and even astonishment, in the minds of every class of observers, still it is necessary they should be seen, to be properly judged of and admired; and, to be seen, it is evident they must be visited. These conditions make the acquisition of universal fame, by the civil engineer, a very slow process. The name of Smeaton has been undergoing it for about sixty years, and is yet far from being universally known out of the profession. It has but recently commenced with that of Telford. But in both cases, it will surely go on so long as the lighthouse stands upon the Edystone, or the bridge is suspended over the Straits of Menai.

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[*] See extract from the minutes of this meeting, Vol. I., p. 399.

But in this, the infancy of fame, the name, like a child, cannot run alone, and should have every help. Suppose, in the excitation of wine and eulogium, one of the successful *convives* at the London Coffee House had run out, with his prize in his hand, and, in "a progress" from St. Paul's to Mile-end, had exultingly shown his medal to every one he had met; suppose, in his route, he had looked into St. Paul's School, had called at the Mansion House, had ran round the Exchange, &c. &c., shouting "Telford, Telford,"—would the tutors, Mr. Hobler, or even his Lordship,—would one per cent. of all the population, "sedentary and ambulant," in that long line, have sympathised with the victor? Would not the mass have required the "Post-office Directory," and hunted through the letter T, to see what occupation, &c., might be attached to the name? This may be said to be an extreme case, and we grant it; but it may serve as a test to try the propriety of using the "Cambyses vein" in themes of common life.

Again, have his countrymen,—have the world,—nothing yet to learn of Thomas Telford? Do they all know what was his profession, which were his works, his honours? How much of this great practician might have been written within "this little O," which would have been read *for the first time* by his countrymen, and afterwards disseminated by them with pride throughout the world! Or does the Institution hesitate to publish that he was their president?—their *first* president? Is it possible their influential members do not like the mention of such stupendous and successful works as the Menai bridge, the Caledonian canal, the Holyhead road, &c., &c.?

Let us see what our neighbours, the French, have done in a very similar case, in that of the engineer of the celebrated canal of Languedoc, *M. de Riquet*. We shall find they have not been so penurious to a departed man of genius, even though they paid for the medal out of their own pockets, and not out of funds provided by the deceased. On one side of a medal lately struck in honour of the memory of M. de Riquet, they have recorded his personal honours, on the other, his greatest professional one; on *both* are his name, *prænomen* as well as *cognomen*. The obverse bears,

P$^{\text{RE.}}$ P$^{\text{L.}}$ DE RIQUET, BARON DE BONREPOS.

The reverse,

A' P. P. DE RIQUET AUTEUR DU CANAL ROYAL DES DEUX MERS.

To the last is added the month and year of the commencement of his great undertaking (October, 1666).

We may also refer to another foreign medal, in the possession of the Institution, one of two gold ones presented to Mr. Telford for his professional services in Sweden,—a medal which states upon its surface, that it was given by the Götha Canal Company, to "THOMAS TELFORD, COMMANDER OF THE ORDER OF WASA*, AND MEMBER OF THE ROYAL ACADEMY OF SCIENCES, FOR HIS ENERGETIC CO-OPERATION IN EFFECTING THE JUNCTION OF THE TWO SEAS†. This was an example worthy of imitation; but perhaps the Institution were alarmed by its being the work of a nation who, utterly unconscious of the terror which such names still spread through more southern regions, have styled their sovereign, on this very medal, "KING OF THE SWEDES, THE GOTHS, AND THE VANDALS!"

Lamenting the dearth of information with regard to the memorable subject of the obverse, we shall merely point out, as a little contrast, the modest care of the artist with regard to his own name. This he has illustrated "fore and aft."

One word only having been devoted to the great master, it will not excite surprise that not a syllable nor a letter has been allowed to his magnificent work. The exergue where an inquirer would naturally look for the verbal illustration of the subject, is usurped by the contemptible and partial abbreviation of *Instit. Civ. Engineers*, &c. This, every one will know, cannot be the name of the bridge; but in the speculations of some, as to what it does mean, is it not probable that it may be supposed that the Institution had something to do with the structure, seeing they are *Civ. Engineers*, and there is nothing to tell what business was followed by the gentleman whose head is on the other side? Ought the Institution to permit the least chance of such an ambiguity? But supposing this inscription in its right place, what, but the most perverse love of "chartered rights," could have preferred the date at which the mere legal convenience of a charter was obtained, to that of the founding of the society, substituting "Incorp. 1828," for "Founded 1818," in the teeth of the instructions of the Council; and thus cutting down the society from the semblance of a vigorous and independent youth of nineteen, descended from known and honest parents though in humble life, and exhibiting it as a foundling of not more than ten, christened by writ of Privy Seal, and permitted to exist only by Royal Will and Pleasure.

Passing from the inscriptions that *are*, we pass to those that *are not*. We have noticed the deficiency with regard to the early friend of the Institution, its first president, its protector during life, and its bene-factor in death. We have also slightly mentioned the injustice done to the Institution itself, but we come now to the party for whom the medal is really struck; the founder and the Institution would not have produced the medal for themselves: a *third* party was the immediate object,—those who, by steady investigation, patient experiment, and careful recording, have contributed to the permanent stock of knowledge on subjects of engineering. How are these provided for in the medal? Where are the "ample room and verge enough" for their names? for their deeds? for the testimonials which may render the voluntary labour of the present time, of essential use to them in after-life? We advise them to "agitate" the Council until they get a satisfactory answer to such inquiries. It would be a shorter course, to agitate the body of the Institution, till they elect individuals who can and will discharge the duties of councillor, and who have no objection to permit the minutes of their meeting, and the journal of their attendances, to be accessible to the inspection of every member. This salutary *exposé* would prevent the president from sitting, as he often has done, *in vacuo.*

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* An honour conferred on Mr. Telford by the late King of Sweden.

† A remarkable coincidence of expression with the inscription on the Riquet-medal, though the Seas in this case are but miniatures of those in the other.

From what we have said, it will be evident, that the medal must be utterly useless to the historian. He may learn that an institution of civil engineers was incorporated in 1828, but where founded, whether at London, Liverpool, Newcastle, or *New York*, the piece saith not. Medals of this character are not documents, but enigmas.

As there is no thought, nor sentiment, nor device, nor allusion,—no invention, nor composition,—no happy adaptation, even of a line, from the ancient stores, nor any striking creation of a new one,—in fact, "no coinage of the brain," whatever there may be of the mint,—it is quite evident this medal will never be coveted by the man of taste.

In the execution of the medal, the refined skill of the artist is evident. In every part of his subject, which he understands, the masterly conduct of his *hand* is conspicuous. If the *head* had been as successfully employed, and if that which has been produced were but a likeness, how invaluable would be the obverse of this medal! Perhaps, for the design of the reverse, he has done nearly as much as was within the compass of his art; and very likely those who know its difficulties will smile when we speak of the atrocious liberty which has been taken, in entirely removing the suspension-rods, in order to expose the lateral faces of the pyramids, of the unfaithful and unfinished architecture of these faces,—of the offensive and ridiculous detail of the masonry, &c. What we principally regret is, that no real friend to the memory of the great engineer, and one who understood the principles of his profession, was in communication with Mr. Wyon; if there had, we should not have seen the vices of the design made the most striking features of the medal; we mean the connexion of the pyramids with the shore, by the useless mass of masonry, and particularly the want of symmetry in the arcades of these masses.

We observe the die of the reverse has failed. The risk of this kind of evil is one to which the medallist is peculiarly exposed. Even in the case under consideration, though we deprecate the existence of the medal for *the purpose* for which it has been struck, we sympathize with Mr. Wyon *in the disappointment.* The medallist stands almost alone in this peculiarity of his process: no care, nor labour, nor excellence, can ensure that the die will stand the hardening; and this operation of hardening must be the last of all, and, therefore, frequently destroys in a moment the labour of months. Unfortunate as it may be, it is clear that a *cracked medal*, even though it were an excellent one in other respects, cannot, with propriety or dignity, be either bestowed or received as a mark of *honorary* distinction!

We regret that we cannot congratulate the Institution of Civil Engineers on what may be called their *début*, as a body, before the public. We know they are strong enough to recover a stumble, but as it is just possible they may not "see themselves as others see 'em," it may be as well to hint to them, that the year which produced the *Introduction* to the *Transactions*,—the *Telford-medal*,—and the *Library Catalogue*[*], will be rather a memorable one in their annals.

ECONOMICAL USES OF SOME SPECIES OF TESTACEA[†].

The study of natural history in this country is evidently progressing; cabinets are formed, and a lively interest is taken in the subject, where, not long since, it was entirely neglected; and the day is nearly past, when specimens are collected merely for their beauty, and preserved solely as ornaments. A scientific interest is now more or less attached to all such objects, and we believe that an earnest desire to study nature, as she exists in the woods, in the air, and in the waters, is daily gaining ground. It is when the science is thus applied, and only then, that natural history is fully useful or interesting; and the more it is so studied, the sooner we reach the truth, and the greater is the pleasure.

The causes of this progress are many and obvious; but we cannot help thinking that the beautiful plates, and the enthusiastic writings, of Wilson, Buonaparte, and Audubon, as well as the valuable public museums in most of our large cities, have done much to promote and force it on[‡]. They have, as it were, forcibly turned the attention of the public to the subject, and laid open to them the stores of happiness and pleasure contained in such pursuits; by intimately mixing in their amusements, they have torn from the face of science the mask of mystery and difficulty with which it was (and, perhaps, in some parts, still is,) unfortunately shrouded; and, by making it familiar, have caused it to be admired and cultivated. But notwithstanding this, natural science has not yet been sufficiently brought home to the every-day feelings and occupations of men. It is still looked upon as a thing apart, not as a thing connected with our education, with our pleasures, or our luxuries; and the knowledge of it, instead of being an assistance in our business, is considered by too many as only an hinderance to it. Professional writers on the subject have, perhaps, rather nourished than helped to dissipate this prejudice: absorbed in the technical and more scientific parts of the subject, they have neglected to show its connexion with the arts, and they have either overlooked entirely, or kept in the background, the application of this knowledge. Natural history is a true science, as are all its branches; and as such, they are the parents and fosterers of the arts. What do not the miner and the landowner owe to the geologist?—the agriculturist to the zoologist and botanist?—and every one to mineralogy? and yet, how seldom is it that we view these pursuits in this light. Were this connexion more frequently reflected on, and the useful results of science more generally brought to notice, we are convinced that many who now neglect it would study it, as conducing to their own progress, as well as to that of their country, in practical knowledge; and those who now idly despise it, would become its warm and active advocates.

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[*] The "*Library Catalogue*," considering all the circumstances which preceded it, is *unique*. We shall probably return to this again, and that with no unbecoming "levity," but in sadness the most decorous. We dare not promise so much with regard to the *Introduction*. In several attempts we have made to read it, we have found its risible power irresistible. It must have been written by "King Cambyses" himself.

[†] From Silliman's *Journal*.

[‡] It may be necessary to explain to foreigners, that these museums, besides being open daily to visiters, on payment of a small sum, are generally used as exhibition rooms for ventriloquists, jugglers, fat men, et hoc genus omne.

As far as regards the cultivation of our minds, the utility of the science cannot be denied. It is from the contemplation of the works of the Creator, that the highest and most glorious thoughts of his majesty and beneficence are drawn; it is from the observation of them that we form our ideas of beauty, of elegance, or of grace; and in them we find a never-ceasing source of admiration, amusement, and instruction. This study enlarges the mind, in a peculiar manner softens the heart, and above all, prompts to a continued dependence on Him who "provideth the raven with his food," and decks the lily with more beauty than even Solomon, in all his glory, was possessed of.

"No man," remarks the great Bacon, "need say that learning will expulse business, but rather it will keep and defend the possession of the mind against idleness and pleasure, which otherwise may, at unawares, enter in to the prejudice of both." The greatest and best in all ages have been celebrated for their knowledge of, and love for, natural history. To mention but a few—Solomon, we are told, wrote a work on the subject: "of trees, from the cedar tree that is in Lebanon, even unto the hyssop that springeth out of the wall; . . . . . of beasts and of fowl, and of creeping things, and of fishes:" Aristotle, the deepest and most able ethical philosopher that perhaps ever existed, has left us a large and most learned work on the same: to Sir Isaac Newton, the Christian is not less indebted than the philosopher; and, in still more modern times, Cuvier is scarcely more celebrated for his researches into, and classification of, nature, than for his political acumen, general learning, and piety. But, perhaps, in no branch of natural history has this connexion been more neglected than in Conchology; the generality of our shells are comparatively so minute, and their uses so seldom obvious to us, that, with a very few exceptions, writers have altogether forgotten to mention the purposes for which society in general is interested in them; and yet, on examining into this subject, we are surprised to find how much we owe to shells, for our luxuries, our pleasures, and our tables; and not only are we, but the ancients were, eminently their debtors. The object of this article is to trace and elucidate those uses, and to show, in connexion with their natural history, in what manner shell-fish, either of old were, or in the present day are, turned to profit. Had the writer been acquainted with any work in which this had been done, he probably would not have attempted it now; but in seeking information on the subject, he was surprised to find how meagre and contradictory were the statements in most of the Encyclopædias, and works of general reference. In the ancient prose writers and poets, in books of history and commerce, in travels and scientific journals, he has found much information, but scattered far and wide, and not likely to be sought after by those who are but little interested in the subject, or unacquainted with the science. This he has endeavoured to compress, as far as was possible with a regard to clearness, and, by carefully giving the references, he has put it into the power of all to look to the original sources, if they desire further information.

Of all the departments of natural history, Conchology appears to be here the most popular: owing to the maritime connexions of this country with all parts of the world, but especially with India and the South Seas, opportunities of procuring specimens are enjoyed, scarcely surpassed elsewhere. And although our marine shells are neither numerous nor generally beautiful, yet those of our rivers are peculiarly interesting, and they are those to which we have most general access. Besides, a collection of shells has advantages over that of every other department; they are always beautiful; they take up but little space, and require no art to preserve them; while, by the Linnæan arrangement, a knowledge sufficient for amusement is easily acquired. But notwithstanding they are in general such favourites, little attention seems to have been paid to their economical uses, and still less, if any, to the improving or becoming possessed of the advantages foreign countries enjoy from them. While our fields and farm-yards are stocked with plants and animals from every part of the world, we have not one shell which has been intentionally introduced for use. In England we are only aware of one attempt to naturalize marine shells, and how that has succeeded we know not; but were proper means used, there is little doubt that many of them would be successful. There, as here, they have animals, and birds, and plants, from every part of the world,—from the most contrary climates, and yet all thriving. The quail and the turkey from this country, the guinea-fowl from the burning plains of Africa, the peacock from Asia, and, still more lately, the kangaroo and emeu from New South Wales[*]; and what is to prevent shells living and breeding in an element by far less subject to change in temperature than the air. The *Ostreæ* and *Pectines*, in particular, might, we feel sure, be successfully transplanted, if proper care were taken[†].

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[*] The following are the foreign animals domesticated in England. The original habitats (with the exception of the two last) are given on the authority of Jenyns, in his *Manual of British Vertebrate-Animals.* From Europe, *Cervus Dama*, *Perdix rubra*, *Cygnus Olor*, *Cyprinus Carpio*, *Esox Lucius (?)* From Asia, *Mus decumanus*, *Equus Caballus*, *E. Asinus*, *Phasianus Colchis.* From Africa, *Mustela Furo*, *Felis maniculata*, *Numida Meleagris*, *Anas Ægypticus*, *Cygnus Guineensis.* From North America, *Meleagris Gallapavo*, *Perdix Virginiana*, *Cygnus Canadensis.* From South America, *Cavia Cobaya.* From India, *Pavo cristatus*, *Gallus domesticus*, *G. lanatus*, *&c.* From China, *Phasianus torquatus*, *Cyprinus auratus.* From Australia, | *Halmaturus giganteus*, (Ill.) *Rhea Novæ Hollandiæ.*

[†] In *Loudon's Magazine of Natural History*, Vol. IX., pp. 572–574, mention

To guess at the causes why this has not been done, were futile; but we may suggest, that it is, perhaps, owing to the very general ignorance on the subject, the employment of fishermen being inferior and ill paid, and the variations of the ocean throwing difficulties in the way, greater than in the introduction of animals and birds on land. The descriptions of the shells have not been arranged systematically; but they stand according to their general interest, or their artificial connexion one with another. It has also been thought best to omit all scientific descriptions, as most of the species are well known, and are to be met with in all books on Conchology.

The Linnæan nomenclature has, with a few exceptions, been followed.

I. MYTILUS MARGARITIFERUS, (Linn.)—*The Pearl Oyster.*

OF all the Testacea, this species is the most valuable, the best known, and enters the most largely into the arts. It not only produces a beautiful and expensive luxury, but the shell is used in the manufacture of various useful and ornamental articles, while the animal serves as food for the inhabitants of those countries where it is most generally found; and although pearls are produced by many other bivalves, yet, from their inferiority in colour and size, this is the only species which can now be said to be of any real importance for this purpose. Anciently, European pearls were extensively used, though never considered equal to the oriental; but as the trade between foreign countries has increased, so has the value of the former gradually diminished, and they are now seldom sought for, and of little comparative worth.

Ancient History.—During the earliest periods of which we have authentic history, the oriental pearl appears to have been known and appreciated; Job, who is supposed to have lived about B. C. 1520, speaks of it as being, in his time, of high value, and much esteemed[*]. Solomon frequently refers to them[†]; and Jeremiah[‡], speaking of the Nazarites of Jerusalem, makes use of a beautiful simile, in describing them as more ruddy than pearls. In the New Testament we frequently meet with them, as inferring great riches and splendour[§].

In Rome, pearls were extensively used, and of great value. Pliny

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is made of two shells which have naturalized themselves in Great Britain. The one is the *Mytilus polymorphus* (Pallas), supposed to have been introduced into the Thames on timber, and now spread through England and Scotland. The other is the *Mytilus crenatus* (Linn.), which must have been brought from Bombay on the bottom of a ship, and has now firmly established itself, in company with the *M. edulis*, in Portsmouth harbour.

[*] Job xxviii. 18. Dr. Hales supposes that Job lived at a much earlier period, and dates his trial, of which we have the account, B. C. 2130. He also appears to have lived in the land of Idumea, and if *so, may* very naturally have been *acquainted* with the pearls of the Persian *Gulf.*

[†] Proverbs iii. 15; viii. 11; xx. 15; xxxi. 10.

[‡] Lamentations iv. 7. The Hebrew word is *Penenim*, and occurs only in the above-cited passages. The Septuagint and the Vulgate translate it as "things hid, precious stones, or ivory." Our English version, with the one exception in Job, always renders it "ruby." David, in his Psalms, makes no mention of it, and, as we do not hear of it till the time of Solomon, when riches from all parts of the world were collected at Jerusalem, we may infer that, previously, the Israelites were not acquainted with it.—See *Calmet's Dic. of Holy Bible*, Art. PEARL.

[§] Matthew xiii. 45. Revelations xxi. 21, &c. &c.

tells us that, in his time, the ladies were not content with using them as ear-drops and rings, but had them embroidered on their dresses and their sandals; they were not satisfied with solely wearing them, but must walk on them, and among them. They were obtained from the Red Sea, or brought to Rome by the Arabian merchants, from the Indian Ocean. They were the most numerous on the coast of Taprobane (now Ceylon), and Toïdis, and round the promontory and city of Perimula; but those from the western shores of Arabia, the Persian Gulf[*], and the Red Sea, were the most perfect and the most celebrated. The mode of capturing them seems to have varied little from that practised at the present day. Divers were employed, who brought them to the surface in nets, and they were packed in jars, with salt, till the animal was decayed. On being taken out, the pearls were found at the bottom of the jars[†]. Among the ancient philosophers, effects were continually attributed to causes the most inconsistent, and the most contrary to nature,—in fact, merely wild or fanciful guesses. Many were of course made as to the origin of these substances. The general opinion appears to have been, that they were formed by drops of dew falling into the shell, for which purpose it periodically rose to the surface; and Pliny gravely informs us, that if the atmosphere was thick at the time, they were dark and clouded; if it was clear, they were white and brilliant. It is singular that the same opinion is found to prevail at the present day among the natives of Ceylon, and very similar to it is the account of their formation recorded in one of the Sanscrit books of the Brahmins[‡]. The same fancy also exists in the interior of Hindoostan[§].

The nacre was manufactured into boxes for the preservation of sweet perfumes and precious ointments.

Of the pearls of ancient times, those belonging to Cleopatra are certainly the most celebrated, and though there is reason for believing that the account of her dissolving one of them in vinegar, and drinking it to Antony's health at supper, is an historical fiction, yet, that a pearl or pearls of great value were in her possession, is pretty certain. In Pliny's time, the two halves of a magnificent pearl, said to have been the fellow to the one destroyed, were hung in the ears of the statue of Venus Genitrix in the Pantheon. This author estimates the value of it at a sum equal to 375,000 dollars. Other persons are also reported to have dissolved pearls, and treated their guests to the same expensive draught. Julius Cæsar gave 48,437*l.* for one, which he presented to one of his mistresses[‖].

There was so much difficulty in obtaining pearls of exactly the same size and colour, that the Roman ladies, about the time of the Jugurthan war, gave them the name of *Uniones*, which appears to have been the first occasion on which this word, now so well known, was applied to shells[¶].

The derivation of the Greek word μαργαρον or μαργαριτης, from which the other countries of the south of Europe have derived theirs, is supposed to be from the Tartar *margion*, signifying a globe of fire, and the Persian *marvarid*, meaning offspring of light[*]. In the present day, the Arabians, Persians, and Turks, use the word *merovorid* for this gem[†]. In Rome they were entailed and handed down to posterity, and if sold, a warrantry of their identity required.

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[*] As early as B. C. 311, the Persian Gulf was famous for them.—*Macpherson's Annals of Commerce*, I. 83.

[†] *Plin. Hist. Nat.* lib. ix. cap. 35.

[‡] *Asiatic Researches*, V. 410, Lond. ed.

[§] *Forbes' Oriental Memoirs*, II. 235.

[‖] *Macpherson's Annals of Commerce*, I. 144.

[¶] *Plin. Hist. Nat.* IX. 35.

Modern history.—The pearls and shells used at the present day are chiefly brought from the Island of Ceylon, where the fisheries are extensively prosecuted, but other parts of the Eastern as well as the Western hemisphere are celebrated for producing them. In India, the chief places are the Island of Bahren, or Beharein, in the Persian Gulf[‡]; Catesa, on the coast of Arabia Felix, and near the city of Nipehoa, on the lake of the same name in Chinese Tartary, as well as in the Red Sea; and on the coast of Japan. A few also are procured near Java and Sumatra. Pearls are brought in great numbers from the Persian Gulf to Bombay[§], but those sold at Madras, and which are found in the Gulf of Manaar, are more highly esteemed in Europe than those procured in this place[‖].

The best shells of commerce are from the Sooloo Islands, situated between Borneo and the Philippines, the shores of which afford the finest and largest shells hitherto discovered. There is also a fishery at Tuticoreen, on the coast of Coromandel, which is held as a monopoly by the British East India Company. They are also found off Algiers, in the Mediterranean; in 1826 this fishery was farmed by an English company, but with what success is not known[¶]. They also inhabit the islands of the South Seas, especially on the coasts of the Paumotu Islands, to which places vessels are sent from New South Wales, and prosecute the fisheries to some extent[**].

In the Atlantic Ocean they are chiefly fished for on the coast of Terra Firma, in the Gulf of Mexico, near the Island of Cubagna, and on the Margarita, or Pearl Islands. The River de la Hache abounds with them[††].

In the Pacific they are plentiful on the Island of Gorgona, and generally along the coast of the Bay of Panama, and being found in shallow water are easily obtained[‡‡]. In the ocean around California, and in the adjacent islands, they are also found in great abundance[§§]. The oriental pearls are, however, by far the finest, surpassing the occidental specimens in colour, clearness, and size.

In the time of Marco Polo (1295), Bagdat, on the Tigris, was the great mart of the world for pearls, and all Europe was supplied from thence[*].

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[*] *Rees's Cyclopædia*, Art. PEARL.

[†] *Calmet's Dictionary of the Holy Bible*, Art. PEARL.

[‡] "We can hardly consider any of the fisheries on the Persian Gulf as belonging to Persia, for although the monarchs of this nation have always claimed the sovereignty of the sea, they have at no period had a navy that could enable them to contend with the Arabic rulers of the opposite coast."—*Sir J. Malcolm's History of Persia, ii. 515.*

[§] *Heber's Narrative*, II. 165. *Am. Ed.*

[‖] *Kelly's Univ. Cambist*, I. 95, note.

[¶] *M'Culloch's Commercial Dictionary*, Art. PEARL.

[**] *Ellis's Polynesian Researches*, Vol. II. pp. 133, 207.

[††] *Chalmers's Universal Dictionary*, Art. PEARL.

[‡‡] *Burney's Chron. Hist. of Discoveries*, IV. 168.

[§§] Natural and *Civil History of California*, I. 49.

In 1506, the Spaniards carried on a large pearl-fishery in the West Indies. About the same time adventurers flocked to California from all parts to enrich themselves with these jewels; and in the beginning of the eighteenth century, numbers from all the western ports of Spanish America congregated there for the same purpose.

Natural history.—The shell of the young of this species is comparatively smooth, but as they grow older it becomes more scaly, rough, and unequal. When full grown, it is sometimes ten or twelve inches long, and the length rather exceeds the breadth. These are thick and ponderous, but the young shells are brittle and slightly eared[†]. It is in the former that pearls are chiefly found, the young ones being either entirely without them, or having them very small and inferior; and the practised fisherman can generally judge in which he will find the largest and most valuable[‡]. They lie in banks or beds, generally near coasts, and in water from three to fifteen fathoms, and, like the other members of the genus, adhere to the rocks and to each other, by the byssus. The older shells lie separately, while the young are attached to the surface of their parents. The byssus is of a dark green colour and metallic hue, and they can move slightly by contracting or extending the muscle to which it is attached. The ancients supposed them to be endowed with peculiar powers of locomotion, describing them as grazing at the bottom of the ocean, with a leader to direct them, &c.[§]; but whether they have any such power is extremely doubtful, at least after attaining the thick shell[‖]. The sexual differences have not hitherto been discovered, although the natives of Ceylon pretend to distinguish the sexes, by the appearance of the shell. Those that are large and flat they call males; those that are thick, concave, and vaulted, they call females; but Mr. Le Beck, who appears to have carefully examined and dissected this animal, declares he was unable to discover any difference[¶].

Like the *Ostrea edulis*, this fish appears to thrive best in a mixture of fresh and salt water. Pearls are always the most beautiful in those places of the sea where a quantity of fresh water falls, as at the mouth of rivers and streams[*], while those produced by the shells growing on rocky bottoms are found to be of a better water than those that lie among sand and coral.

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[*] *Macpherson's Annals of Commerce*, I. 456.

[†] *Dillwyn's Descr. Catal. of Shells*, I. 302.

[‡] *Asiatic Researches*, V. 393—411.

[§] *Pliny*, IX. 35.

[‖] Mr. Montgomery Martin makes the following remarks on this shell, but from his very obvious ignorance of natural history, and the general inaccuracy and haste of his observations in this department, they are very little to be relied on. We however give them, as from a modern and widely-circulated work, and not entirely without its merits, although abounding in faults. "At certain seasons, the young oysters are seen floating in masses, and are carried by the currents round the coasts (of Ceylon). They afterwards settle and attach themselves by a fibre or beard on coral rocks, and on sand; they adhere together in clusters; when full grown, they again separate, and become locomotive. The pearls enlarge during six years, and the oyster is supposed to die after seven years."—*Martin's History of the British Colonies*, 2d ed. I. 522, note.

[¶] *Asiatic Researches, ut supra.* Dr. Kirtland remarks the same with respect to those species of the family of *Naïades* of Lamarck, which are found in the waters of this country, and he is "persuaded that each sex possesses a peculiar organization of body, associated with a corresponding form of the shell, sufficiently well marked to distinguish it from the other."—*Silliman's American Journal*, Vol. XXVI. pp. 118, 119.

Of the many suppositions as to the cause of pearls, that of Réaumur is the most probable, and in the present day generally adopted. He supposed them to be owing to a disease in the fish, as calculi in mammalia, and to arise from a ruptured or morbid state of the vessels provided for the secretion of the materials of the shell: most experiments and observations go to prove the truth of it[†].

They are found in the body of the animal, and not confined to any particular part. Some contain but one, some a multitude of small specimens, known by the name of *seed-pearls*, while in the shell itself, and protruding above the surface, are frequently circular nodules resembling them; as these must be cut, and on one side are flat, they can only be used for setting, and of course are of less value. They are technically termed by jewellers the *wens of pearls*.

There is a common opinion that the animal may be forced to produce the gems by artificial means. The inhabitants of the shores of the Red Sea were said to have wounded them, and returned them to the water; and the Chinese, we are told, insert beads of the nacre into the shell, to be covered by the animal with the perlaceous substance[‡]. But these accounts, from many obvious reasons, are improbable, and we have not been able to meet in modern histories with any authenticated instance of its having been done. In river-shells it might be more easily effected; and we shall have to refer to it again under the description of *Mya margaritifera*. They sometimes grow so large as to hinder the shell from shutting; in which case the fish dies[§], and they are comparatively not uncommon of the size of a small bullet.

On analyzation, perlaceous shells appear to be formed of animal matter, applied stratum upon stratum, with carbonate of lime between them; each membrane has a corresponding coat or crust of carbonate of lime, secured by a new membrane; and each coat of lime is so situated, that it is always between every two membranes, beginning with the epidermis, and ending with the last-formed internal one. The animals which inhabit these stratified shells increase their habitation by means of a stratum of carbonate of lime secured by a new membrane, and as every additional stratum exceeds in extent that which was previously formed, the shell becomes stronger in proportion as it is enlarged; and the growth and age of the animal may be denoted by the number of strata which concur to form the shell. Pearls prove also to be similar in composition to the nacre, and they appear to be formed of concentric coats of membrane and carbonate of lime. The wavy appearance and iridescence of mother-of-pearl, and of the pearl itself, are evidently the effect of the lamellated structure and semi-transparency. On dissolving the lime by acid, the animal frame-work of the shell is left exposed [*].

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[*] *Bruce's Travels to discover the Sources of the Nile*, VII. 322.

[†] *Transactions of the French Academy.*

[‡] See *Rees's Cyclopædia*, *Encyclopædia Americana*, &c. &c. Art. PEARL. Beckmann, in his *History of Inventions*, Vol. II. p. 5, third Lond. Ed., seems to have been the first in more modern times to have given extended publicity to this opinion. He quotes as his authority a German work, and adds, that "the truth of this information cannot be doubted." That it *might* happen, we do not mean to deny, but sufficient authority is wanting to prove it, and many powerful reasons are against it. How and when does the reader suppose the pearls were inserted? on a string containing five beads; and the clever Chinese caught the shell when it rose to the surface in the spring to enjoy itself! Of course next year it had to be caught *again to get* at them, but how, we are not *informed*. He has, however, been servilely *copied* by most subsequent writers on this subject, with the exception that he expressly says the shells are fluviatile, while his copyists confuse them with, or describe them as, the present marine species. Beckmann confesses that some experiments of the kind, made in Bohemia, were without success. Postlethwaite gives an account in some respects similar, but still less creditable, from a Chinese work. The pearl was made of several materials; among others, nacre and bruised pearls, and the shell was kept in a basin, to be daily fed on medicinal and gummy roots. He himself allows that it does not appear probable. (See *Postlethwaite's Dictionary*, Vol. II. Art. PEARL.) It is time, however, that such fables were exploded, and left out of works professedly scientific, and bearing on the title-page the name of some learned editor, assisted, as we are told, by "eminent professional gentlemen."

[§] *Chalmers's Universal Dictionary*, Art. PEARL.

Fisheries.—In the older writers, the following mode of catching this fish is given, and though it differs in some respects from the latest accounts, yet the difference may probably be owing to the lapse of time, or from being practised at different places. It however refers to the Island of Ceylon, though we are not told the exact part.

There were two seasons when the shells were particularly sought after, during March and April, August and September. The vessels used were of two sizes, the larger carrying two divers, the smaller only one. In the morning they set sail for the banks with the land-breeze, which at these seasons blows regularly, and, on arriving there, anchored. The diver bound a stone to his body, about six inches thick and one foot long, to enable him to walk against the water, and another of from twenty to thirty pounds weight to his foot in order to sink him; around his neck was a net to hold what he collected, and he was provided with strong gloves, or a short iron rake, to tear the shells from the rocks [†]. His ears and nostrils were filled with cotton, and a sponge dipped in oil was fastened to his arm that he might occasionally breathe without inhaling water [‡]. Round his waist was a cord to pull him up again when he grew tired. Thus equipped, he leaped overboard, and collected as many shells as he could while he remained under water. We are told they sometimes remained below fifteen to twenty minutes, but this seems a very gross exaggeration, from two to seven minutes being the general period in the present day; they repeated this while they stayed as often as they could. In the evening the boats returned with the sea-breeze, which then blows on land. On the beach the fishers dug pits four or five feet square, and throwing in the oysters, raised heaps of sand over them to the height of a man, so that at a distance they looked like an army ranged in order of battle. When the animal was decayed, these were opened, and the sand passed through sieves to collect the shells. These again were passed through nine sieves to assort the qualities, and the smallest sold as seed-pearls.

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[*] *Philosophical Transactions of the Royal Society of London*, XVIII. 554. Abrid. Ed. 1807.

[†] *Chalmers's Dictionary*, Art. Pearl.

[‡] *Encyclopædia Americana*, Art. Pearl. "According to the depth of the water, the seal takes into its stomach a quantity of pebbles, as ballast, as it were, being obliged to sink itself. To enable it to dive so admirably, it collects a quantity of blood on the right side, *the same as is found to be the case with those persons employed in diving for pearls.*"—Dr. Riley, at the meeting of the British Association at Bristol, 1836, reported in the *Literary Gazette*, Sept. 3.

The manner of catching them in the West Indies differs very little from this mode. The employment is considered very unhealthy, and the divers are continually in danger from sharks. Among the Colchi, during the first century of the Christian era, so dangerous was this business considered, that condemned criminals were entirely and solely employed in it *.

Since the British have held possession of the Island of Ceylon, the pearl-fishery has been a monopoly in the hands of government. It is now a more important business, and carried on with greater skill and tact. About twelve miles from Manaar, and in the gulf so called, lies the Bay of Condotchy, on which is a small town of the same name. Off here the fishing-banks lie, and extend several miles along the coast from Manaar Sound to Arippo. The principal one is about twenty miles out to sea. At this town, scarcely inhabited except during the fishing time, all the boats collect in the beginning of the season; few of them belong to the island, and most of them are brought annually from the coast of Coromandel and Malabar. The fishing is confined to one season, and begins generally about the second week in February. The first thing that is then done is to sell by auction to the highest bidder the right of fishing for the season, and he either catches all himself, hiring the boats, or lets out the privilege to others. The fishing seldom continues above thirty entire days, for though it may be carried on till the 15th of April, yet owing to holidays, storms, and other causes, they seldom work more than this time in the two months. It is found necessary to leave the banks from four to seven years to recruit, so that parts only are annually disturbed. Each boat carries twenty-one men, ten of whom are divers, and one the *Sandel* or head boatman. At ten o'clock at night, on a signal, the boats leave Condotchy together, with the land-breeze, and reach the banks about day-light. They immediately begin diving, and continue till the sea-breeze sets in. The divers are very expert, and go down by fives, so that there is always one-half resting. They require no assistance, except a stone to their feet, a net, and a rope. From their earliest infancy they are accustomed to the exercise, and fearlessly descend to the bottom, at a depth of from five to ten fathoms, in search of the treasure. The time they generally stay below is about two minutes, but some have been known to exceed seven. To continue longer than this is supposed to be impossible. On coming up they generally discharge water from the nose and mouth, and occasionally blood. The cargo of one boat may amount to thirty thousand oysters, if the divers have been industrious and successful.

On landing them, some merely throw them on mats to rot in the open air; others bury them in enclosed pits about two feet deep, till the animal has dried up. For sorting them they make use of brass plates, perforated with holes of different sizes. The piercing is accomplished by an instrument peculiar to the natives, and which they use with much

M'Pherson, I. 172.

ingenuity. It is a conical piece of soft wood, on legs, with pits for the larger pearls. The smaller ones are beaten in with a wooden mallet. The drilling instruments are iron spindles of various sizes, and are turned round in a wooden head by means of a bow, while the pearl is occasionally moistened with a little water[*].

In the Persian Gulf there are two seasons, but shells are fished for chiefly in July, August, and September. The divers use a piece of horn to compress their nostrils, and bees' wax to stop their ears[†].

In California, the banks (which are there called *Hostias*) lie in three or four fathoms of water, and the fishery is carried on by divers, but owing to the little depth at which they are found, with neither labour nor difficulty[‡]. The fishery at Colombo, in Ceylon, was at one time very productive, but some years ago it entirely failed; and though it has been lately (1825) resumed, the success has been small[§]. These sudden failures occasionally occur in most banks[||], but we have not met with any satisfactory explanation of the cause. In the Red Sea this species holds the first rank among pearls, and is called the *Lulu el Berber*—*i. e.* the pearl of Berber, or Beja, in the country of the shepherds[¶].

Uses.—Besides the pearls, the shell and fish are both used. The former in the manufacture of knives—for inlaying, in the constructing of ornaments, &c.; and the latter as food. The nacre is generally separated from the external part by the lapidary's mill or aqua fortis, and cut up by instruments made expressly for the purpose[**]. The inlaying of mother-of-pearl has been brought to high perfection at Jerusalem; great quantities of the shell are carried daily from the Red Sea to that place, and of these, all the fine works—the crucifixes, the wafer-boxes, and the beads are made, which are sent to the Catholic countries of South America[††]. We may suppose the Romans were ignorant of this art, as Pliny makes no mention of it. The fish is eaten by the lower classes of the Singhalese, either fresh in their curries, or cured by drying. In this latter state they are carried to the coast of Hindoostan, and disposed of there[‡‡]. In the Island of Gorgona, also, they are used by the Indians and Spaniards, and hung on strings to dry. If eaten raw they taste coppery; but when boiled are considered good[§§]. The aborigines of California were ignorant of the use of the pearl, but they used the fish, throwing it on the fire to get at it and cook it[||||]. It is said, also, that on the discovery of Mexico, the Spaniards found these gems in use and esteemed by the Indians, but that from the same manner of killing the animal their colour and lustre were destroyed. In the Society Islands, the Indians manufacture their fish-hooks out of this shell, the glitter of which serves instead of bait[¶¶].

Besides being used as an ornament, pearls were formerly in high estimation as a medicine, and given in cordial potions; but as they differ in no respect from any other calcareous earth, they have been long neglected, and are now, we believe, never administered[*]. The ladies, however, still use a cosmetic under the name of pearl-powder, but which is, notwithstanding, understood to be metallic.

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[*] *Asiatic Researches, ut supra.*

[†] *M'Culloch*, Art. PEARL.

[‡] *History of California*, I. 49.

[§] *Heber*, II. 165.

[||] *Dictionnaire Universelle de la Géographie*, par J. Peuchet, Art. CEYLON.

[¶] *Bruce*, VII. 323.

[**] *Ency. Ameri.*, Art. NACRE.

[††] *Bruce*, VII. 322.

[‡‡] *Asiatic Researches, ut supra.*

[§§] *Burney*, IV. 168.

[||||] *Hist. California*, I. 49.

[¶¶] *Ellis's Polynesian Researches*, I. 146.

The most remarkable pearls of modern times are, one which was in the possession of Philip II., in 1574, as large as a pigeon's egg; one mentioned by Tavernier in the hands of the Emperor of Persia in 1633, and which was purchased of an Arab for the extraordinary sum of 110,400*l.*; and that of the Emperor Rudolph, mentioned by Boëtius, called *La Peregrina*, of the shape of a pear, weighing thirty carats[†]. In Europe, in common with all jewels, they are sold by the carat. In Asia, the weight differs in different states. At Bombay and Madras pearls are valued by two kinds of weights, real and nominal; by the former they are weighed, by the latter sold[‡]. In the former country, pearls of a "white water" are most sought after, but the Indians and Arabs prefer those of a "yellow water[§]."

Artificial pearls, of which the Romans appear to have been ignorant, are made of thin glass beads lined with the scales of, or a white powder from the belly of the bleak, (*Cyprinus Alburnus*, Linn.,) and filled with wax. To obtain a pound of scales, four thousand fish are necessary, and these do not produce four ounces of the *essence d'orient*, as the perlaceous substance is termed. At St. John de Maizel in the Challonnois is a manufactory in which ten thousand pearls are made daily[‖]. From the facility and accuracy with which these are manufactured, the price of the real article in modern times has much declined. A handsome necklace of Ceylon pearls smaller than a pea, costs from seven hundred and fifty to thirteen hundred and fifty dollars; but one of pearls about the size of peppercorns may be had for about sixty-five dollars; the pearls in the former selling at five dollars each; those of the latter at thirty-seven cents.

The importation of mother-of-pearl shells in England, in 1832, amounted to seven hundred twenty-one thousand five hundred and twenty-seven pounds weight[¶].

[To be continued.]

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[*] *Hooper's Med Dic.*, Art. MARGARITA.

[†] *Chalmers*, Art. PEARL.

[‡] *Kelly's Univ. Cambist*, Vol I. pp. 92, 95.

[§] *Rees's Cyclopædia*, Art. PEARL.

[‖] *Beckmann*, II. 16.

[¶] *M'Culloch*, Art. PEARL.

THE PINE-APPLE PLANT AND ITS FIBRE.

[We are indebted for the following account to the spirited individual who is endeavouring to call the attention of our West India proprietors to this new employment of labour and capital. This gentleman explained his views on the subject to a select party of scientific persons and others, who met at the Gallery of Practical Science, for the purpose, on June 8th.]

THIS plant, which has hitherto been valued solely as ministering to the luxuries of the table, has lately had a new interest attached to it from the discovery of a fibre contained in its leaves, possessing such valuable properties, that it will, in all probability, soon form a new and important article of commerce.

This fibre is found, on comparison, far to surpass in delicacy of texture, those materials which now form the basis of our woven manufactures: of these the principal are silk, wool, cotton, and flax. Silk is a continuous fibre, often extending, without interruption, to the length of 1000 feet; viewed under the microscope, it is found to be perfectly cylindrical, beautifully smooth, glossy, and transparent. The best kind of prepared silk varies from $\frac{1}{1700}$th to $\frac{1}{2000}$th part of an inch in diameter. Wool, on the other hand, is a rough cylindrical fibre, which appears as if plated with irregular scales, the edges of which overlap each other; though invisible to the naked eye, its roughness may be easily detected, by drawing a fibre between the fingers, in a direction from its end to its root. It varies greatly in both length and size, the diameter of very fine wool being from $\frac{1}{700}$th to $\frac{1}{1200}$th part of an inch. Cotton, being the down of a seed-pod, has peculiar characters, a thin continuous tube, presenting the appearance of a flattened and twisted cylinder, arising, probably, from the compression which the fibre undergoes before the opening of the pod: its diameter is about the $\frac{1}{1000}$th part of an inch. Flax, in many respects, differs from the foregoing materials; a filament of silk, wool, or cotton, admits of no division, while, on the other hand, each filament of flax is a fasciculus or bundle of fine fibres, connected by thin membrane, and the natural gluten of the plant. These fibres may be termed ultimate, as they admit of no further subdivision: they are cylindrical tubes, disposed parallel to each other, and having the appearance of innumerable joints; their diameter varies, in different flaxes, from $\frac{1}{500}$th to $\frac{1}{2500}$th part of an inch.

The fibres of the pine-apple plant are also disposed in fasciculi, each apparent fibre being an assemblage of fibres adhering together, of such exceeding delicacy, as only to measure from $\frac{1}{5000}$th to $\frac{1}{7000}$th part of an inch in diameter; viewed under the microscope, they bear considerable resemblance to silk, from their glossy, even, and smooth texture. They appear altogether destitute of joints, or other irregularities, and are remarkably transparent, particularly when viewed in water: they are very elastic, of great strength, and readily receive the most delicate dyes. This last fact appears singular, when we bear in mind the resistance, if we may be allowed the expression, which flax offers to dyes. With much trouble, and by long processes, flax will receive a few dark and dingy colours: all light and brilliant ones it wholly resists, they do not enter the fibre, but merely dry upon it externally, and afterwards easily peel, or rub off,—in short, it may be said to be *painted*, and not *dyed*.

The preparation of the pine-fibre is exceedingly simple. If a leaf of this plant be examined, it will be found to consist of an assemblage of fibres running parallel from one extremity of the leaf to the other, embedded in the soft pabulum. All the process necessary is to pass the leaf under a "tilt hammer," the rapid action of which, in a few seconds, completely crushes it, without in the slightest degree injuring the fibre, which remains in a large skein, and then requires to be rinsed out in soft water, to cleanse it from its impurities, and be afterwards dried in the shade. So simple and so rapid is the process, that a leaf, in a quarter of an hour after having been cut from the plant, may be in a state fit for the purposes of the manufacturer, as a glossy, white fibre, with its strength unimpaired by any process of maceration, which, by inducing partial putrefaction, not only materially injures the strength of flax, but also renders it of a dingy colour.

The pine-plant abounds both in our East and West India possessions, and may be easily propagated from the crown; the offsets from round the base of the fruit, which often amount to upwards of twenty in number; and from the young plants which spring from the parent stem; its cultivation requires but little care or expense, and the plant is of such a hardy growth, as to be almost independent of those casualties of weather, which often prove so detrimental to more delicate crops—it is one of those plants which Nature has scattered so profusely through tropical regions, whose leaves are thick and fleshy, to contain a large supply of nourishment, and covered by a thick, glazed cuticle, which admits of so little evaporation, that many of the tribe will thrive upon a barren rock, where no other plant could live. From the large portion of oxalic acid which the leaves also contain, no animal will touch them, and they are, therefore, exempt from the tresspasses of cattle, &c., indeed, no greater proof of the hardiness of the plant can be given, than the fact, that in many places where lands have been under tillage, and have afterwards been abandoned, and allowed to return to a state of nature, the pine-apple plants form the only trace of former cultivation; every other cultivated plant has died away before the encroachments of the surrounding wood, while they alone have remained increasing from year to year, and have spread into large beds.

In adverting to the present state of our West Indian colonies, we cannot but think that the discovery of this fibre will prove to them a most valuable acquisition. The small amount of labour and of capital requisite for the cultivation of the plant, its hardy growth, its abundant produce, the facility with which the fibre may be prepared from its leaves, the trifling cost at which it can be brought to market, and the value attached to it, as an article of commerce, by those manufacturers to whom it has been submitted, seem fully to justify the opinion that it is calculated to open a new source of wealth to the colonies, and become a staple and valuable article of British manufacture.

SIMULTANEOUS METEOROLOGY.—No. II.

TABLE of HOURLY METEOROLOGICAL OBSERVATIONS, made during the 21st and 22nd June, 1837, at BLACKHEATH ROAD, near Greenwich, about four miles and a half S.E. of London, *by and under the superintendence* of J. H. BELVILLE, in conformity with the instructions circulated by the South African Literary and Philosophical Institution.

DATE.	HOUR.	BAROMETER.	THERMR. Attached	THERMR. In open air.	WIND. Direction.	WIND. Strength	Prop. of Cloud.	STATE OF THE ATMOSPHERE, CLOUDS, &c.
		Eng. In.	Fah °	Fahr. °				
Wednesday, June 21, 1837.	VI. A.M.	30·028	67	57·1	W.	1	10	*Stratus.* The *minimum* of the self-registering thermometer before sunrise, 54·5°.
	VII.	30·046	67	59·0	W.	1	10	Ditto.
	VIII.	30·052	68	62·8	W.	1	5	*Stratus* into *cumulus.*
	IX.	30·054	68	67·8	W. by S.	1	5	*Cirro-cumulus.* Hazy.
	X.	30·075	69	69·0	W. by S.	1	4	*Cumulus.*
	XI.	30·085	70	67·0	S.W.	1	3	Ditto.
	XII.	30·085	71	69·9	S.W.	2	3	Ditto. Brisk wind and cool.
	I. P.M.	30·098	72	70·5	S.W.	2	3	Ditto. Ditto ditto.
	II.	30·098	73	73·4	W.S.W.	1	6	Ditto.
	III.	30·100	73	73·6	W.S.W.	1	7	The same as before.
	IV.	30·103	72	71·2	W.S.W.	1	8	Dense *cumulo-stratus* has prevailed since last observation.
	V.	30·110	71	71·1	W.	2	5	Getting clearer and lighter.
	VI.	30·134	72	69·1	W. by S.	2	5	Getting more clear.
	VII.	30·150	74	68·2	S.S.W.	1	6	*Cirro-cumulus* to the S.
	VIII.	30·164	73	66·5	S.W.	1	8	Ditto ditto collecting.
	IX.	30·176	72	62·9	S.W.	1	1	Getting clearer again towards the N. & W.
	X.	30·198	71	60·6	W.S.W.	1	4	Clouds again collecting to N. of zenith.
	XI.	30·200	71	59.5	W.S.W.	0	4	Nearly cloudless. *Moon rises in a clear horizon.*
	XII.	30·205	71	58·0	none.	0	1	Cloudless. Calm and serene throughout the night.
Thursday, June 22.	I. A.M.	30·210	71	54·5		0	0	Ditto.
	II.	30·231	71	53·6		0	0	Ditto. A low creeping mist on the distant hills to the S. a little before sunrise.
	III.	30·240	71	53·0		0	0	Ditto. The *maximum* of the self-registering thermometer at sunrise, 50·0°.
	IV.	30·246	70	51·7	W.	0	0	Ditto.
	V.	30·250	70	54·0	W.	0	0	Ditto.
	VI.	30·262	69	55·8	W.	1	0	Ditto.
	VII.	30·281	69	59·0	S.S.W.	0	2	Light *cirri.* A breeze coming on.
	VIII.	30·293	68	63·5	N.W.	0	3	Great obscurity from mist and smoke.
	IX.	30·296	68	66·0	N.	1	1	Wind changes to N. Mist increases.
	X.	30·298	69	70·0	N.	1	1	A few *cumuli* appear through the mist.
	XI.	30·300	70	71·5	S.W.	1	2	So misty that the sun scarcely produces a shadow.
	XII.	30·301	71	73·0	W.	1	3	*Cumuli* & *cirro-cumuli.* Mist and smoke still prevail.
	I. P.M.	30·301	71	69·0	N.	1	4	Ditto.
	II.	30·302	72	72.0	N.E.	1	3	Ditto.
	III.	30·300	72	71·0	N.E.	0	4	Ditto.
	IV.	30·300	72	71·3	E.	0	5	Clouds and mistiness rather going off.
	V.	30·298	73	71·2	E.	0	4	A little clearer.
	VI.	30·300	72	69·8	E.	0	4	No variation since the last observation.

NOTES.—The two days rather cloudy and misty; the intervening night clear and serene. The whole of the clouds came up from the *westward.* The *mist* prevailed chiefly in the *lower* atmosphere.

A little before midnight, a large fire broke out to the N.W. between Deptford and Rotherhithe, at a spot about two miles in a direct line from the Meteorological Station, from which a dense column of smoke rose nearly perpendicular to a great elevation. *No perceptible current of air could be detected.* The night ended remarkably still and calm.

The time was taken from a good clock, keeping mean time, *rate* scarcely perceptible during the observations; *error* obtained by observation of the Ball-Drop, Royal Observatory, Greenwich.

The Barometer has an elevation of about 40 feet above the River Thames.—Makers, Watkins and Hill, London; reads off about ·025 lower than a standard barometer lately made by the same artists. The Thermometer has a northern aspect—Maker, Dollond.

The *strength* of the wind is indicated by the figures, thus – 0 means *no wind perceptible;* 1, *very light breeze;* 2, *strong breeze.*

In the column headed *Proportion of cloud,* 0 signifies quite clear; 10, no blue sky visible; 5, sky *half* covered, &c.

MISCELLANEA.

A "Decillionth."—Homœopathy.

The new medical doctrine, designated Homœopathy, appears to be spreading in England: at least, it is now a frequent subject of conversation and inquiry. As taught and practised by its honest professors, it is by no means uninteresting to the general observer, whether he be or be not a believer in its efficacy. We are not about to dispute the principles, nor examine the evidence, upon which the system is based, but, independently of these, there is a part of the practice of the homœopathists which every son and daughter of Adam must wish to be true, and for this reason,—that every one of them is at some time or other a medicine-taker; and at no time whatever, if they be sane, a *willing* recipient. The practice we refer to as so generally interesting, is the inconceivably small quantity of medicine prescribed in every case without exception. Any quantity so large as the visible fraction of a grain, is never *exhibited*. In the homœopathic pharmacopœia, the "*decillionth of a grain*" of a medical substance is a usual dose, and even a small part of this fraction is sometimes prescribed.

The term "decillionth" is, perhaps, as new to most of our readers as it was to us; we had never exceeded "trillions" in our widest excursions from the numeration-table, and we, therefore, from a love of correctness, proposed to obtain a notion of the quantity denominated "the decillionth of a grain." Never was "the *agony* of precise conception" more exquisitely acute. We had not the slightest previous idea of the unapproachable minuteness of the magnitude in question, and were absolutely lost in our attempts to arrive at the proposed subdivision, even when armed with "millions" as our divisors. We, therefore, desisted in despair, scratching out the formula graphically upon our blotting sheet, portraying the grain, (say of opium,) rolled into a spherule, which boasted a diameter of two-tenths of an inch—actual admeasurement, and placing beneath it the formidable fraction, certainly no *vulgar* one, whatever the schoolmaster may assert.

$$\frac{1}{1000000000000000000000000000000000000000000000000000000000000}$$

Briggs, the celebrated mathematician, who stands alone as having left behind him a few numbers to the fifty-ninth decimal place, is here beaten by a *length!*

If, after all our labours, we were asked about how long a grain would last for the prescriptions of an homœopathist in very extensive practice, we should confess our utter inability to give an answer: we could say, indeed, that the population of the world, taking it at seven hundred millions, and every individual so mortally sick as to require a dose per second for twenty years *successively, would* produce no sensible *diminution in our* spherule; nay, if the *earth contained* a thousand millions, and if there were a thousand millions of such worlds, and if each inhabitant lived for a thousand years, they might each take a dose per second during their whole existence, and more than half the grain would still be left unconsumed! What a prospect for the drug-trade! All but brewers' druggists will be annihilated. The golden head of Glauber would itself become a drug, and Apothecaries' Hall be shut up; for all drugs will then be *drugs*, which it is said they often are not in the present day. This is not banter; let those who think it is, try at a "decillionth," as we have done, and if they arrive at it, and exhibit it physically, even with the aid of the oxyhydrogen microscope of the Adelaide-street

Gallery, granting it to possess the marvellous powers which were wont to be attributed to it by a *ci-devant* exhibitor,* they will have done a deed which would be very like bringing "nothing" into existence.

Still the genuine homœopathist prescribes "a very small part" of this decillionth of a grain, and believes in the virtue of his dose. Nay, one of the great teachers of the doctrine, in his practical instructions urges "that the degree of attenuation be faithfully observed, for the energy and the duration of the action of the dose are different in every degree, and that consequently the substitution of one of them for another can never be a matter of indifference."!

The mode by which this excessive attenuation is believed to be attained may not immediately suggest itself to persons unacquainted with the subject. The operation, however, is a simple one, and though the result will certainly be imperceptible to the senses of non-believers in homœopathy, they cannot dispute the rationale of the process. *Dissolve a grain of the substance to be taken in one hundred drops of alcohol; to one drop of the solution add ninety-nine drops of alcohol; then to one drop of this latter solution add another ninety-nine drops of alcohol, and so on, to the number of thirty times.* If we admit that a drop of the first solution contained the $\frac{1}{100}$th of a grain of the dissolved substance, and which cannot be disputed; then a drop of the second will contain the $\frac{1}{10000}$th,—one of the third $\frac{1}{1000000}$th, and one of the thirtieth will contain no more than the "decillionth" of the grain!

Upon quantities of this extremely minute magnitude, and even upon very small fractions of such extremely minute quantities do the professors of the homœopathic system depend for the cure of all the diseases and functional derangements that "flesh is heir to."!

Hopes deferred. Second Series.

A JOURNAL of the "Patents for Inventions' Bill." Session 1837!

14th Feb.

Patents for Inventions.—Bill to alter and amend the Patent Laws, and for better securing to individuals the benefit of their inventions, *ordered* to be brought in by Mr. Mackinnon* and Mr. Baines†.

15th Feb.

Patents for Inventions' Bill, "to amend the practice relating to Letters-Patent for inventions, and for the better encouragement of arts and manufactures," *presented* and read 1°; to be read 2° on *Wednesday*, 1st of March, and to be *printed*.

1st March, (14 *Days from first Reading*.)

"Patents for Inventions' Bill—Second Reading *deferred* (for *forty-two days*), till *Wednesday*, 12th April."

12th April, (56 *Days from First Reading*.)

"Patents for Inventions' Bill—Second Reading *deferred* (for *thirty-five days*), till *Wednesday* 17th of May."

17th May, (91 *Days from First Reading*.)

No notice of the Bill in the "Votes and Proceedings of the House of Commons" of this day.

1st June, (106 *Days from First Reading*.)

"Patents for Inventions' Bill—Second Reading (*deferred for twenty days*), on *Wednesday* 21st June."

21st June, (126 *Days from First Reading*.)

"Patents for Inventions' Bill" stands as No. 2. among the "Dropped Orders" of this day. Could a more deplorable picture be exhibited?

Cambridge Observatory Operations in 1836.

"THE two principal objects kept in view in the system of observations now carried on at the Observatory are the same as in former years: viz., to maintain an unbroken series of observations of the sun, moon, and planets, and to exhibit the results of these observations completely reduced and compared with tables, so as to be quite ready for any scientific application.

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* . . . . "The STING OF A BEE,
And the *one thousandth part* of the LEG OF A FLEA,
TEN MILLION OF MILLION times is magnified."

So sang the POET OF THE ADELAIDE-STREET GALLERY ESTABLISHMENT, Mr. *****, but he slily added *(aside)*,

I *knew* NEW *knew* better, so *knew* that NEW lied.

Mr. New was the exhibitor of the Microscope.

* Member for Lymington.

† Member for Leeds.

"The subordinate observations are, those of moon-culminating stars to serve in conjunction with observations at other stations for determining geographical longitudes; observations of occultations of fixed stars and planets by the moon, and eclipses of Jupiter's satellites for the same purpose; micrometer measurements of differences of north polar distances with the equatoreal, for the more correct determination of the parallaxes and apparent diameters of certain of the heavenly bodies; and observations with the transit and mural circle for assigning the exact positions of stars occurring incidentally in the other observations.

"From the commencement of the current year to the present time, the established system of observations has been followed up, with the addition, in the months of February and March, of an extensive series of observations with the circle and equatoreal of Mars and of stars near him, with the view of determining, in conjunction with contemporaneous observations in the southern hemisphere, the parallax of that planet. These observations were made under favourable atmospheric circumstances.

"The delay in the completion of the Northumberland telescope has been occasioned by the illness of Mr. Simms. It is expected to be erected in the course of the month of June."

The observations, since the 24th of March, 1836, have been made under the direction of Prof. Challis, who then succeeded Prof. Airy.

The volume containing the observations from the above date to the end of the year, will be ready for publication next month (August).—*Report of the Observatory Syndicate*, May 20, 1837.

Prize Subjects, 1837. *Institution of Civil Engineers.*

THE Council of the Institution of Civil Engineers have lately printed and distributed the following resolutions, respecting the "Annual Premiums[*]," provided for by the late Mr. Telford.

"1st. The premiums shall be honorary, or honorary and pecuniary, or pecuniary simply.

"2nd. The honorary premiums to consist of a Telford medal struck in gold, silver, or bronze.

"3rd. The honorary and pecuniary premiums to consist of a Telford medal, and such sum of money as the circumstances of the case may seem to require.

"4th. That all communications made to the Institution will be considered as subjects for the premiums, no distinction being made in awarding the premiums between the communications of members, associates, or others in no way connected with the Institution, *whether natives or foreigners.*

"5th. The Council will not allow exertions in aid of the Institution to go unrewarded; they consider that those who forward the great objects which the Institution have in view, as entitled to the marks of distinction which these premiums will confer."

In the resolutions of a Council, held in February, 1835, the following more specific instructions, as to the subjects for which the premiums would be given, will be found.

"1. Descriptions, accompanied by plans and explanatory drawings, of any work in civil engineering, as far as absolutely executed, which shall contain authentic details of the progress of the work[†].

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[*] See Vol. I., p. 398.

[†] Smeaton's account of the Edystone Light-house was pointed out as an example by this Council, but in the recently-distributed communication Smeaton is not mentioned, and there is an unfortunate substitution of Mr. Timperley's account and drawings of the Docks executed at Hull by Mr. James Walker. It is said, in a most ambiguous note, "that the manuscript of this communication is a most beautiful model for imitation." We consider the substitution *unfortunate*, because, to say nothing of its arrangement, its perspicuity, its fullness without overflow, Smeaton's Edystone has long ago found a place in every library, and is, therefore, easily accessible to every person, whereas, Mr. Timperley's account, however meritorious, has been published but a few months, and that only in the first volume of the Transactions of the Institution. The note is *ambiguous*, because "manuscript," according both to Johnson and the usual acceptation, could not possibly be found in a *printed* volume, and, therefore, a candidate at a distance might conceive that the manuscript, recommended as so worthy of imitation, could be consulted only in the archives of the Institution, and, therefore, believe it to be beyond his reach. An anxious candidate so circumstanced would really deserve pity.

"2. Models or drawings, with descriptions of useful engines and machines; plans of harbours, bridges, roads, rivers, canals, mines, &c.; surveys and sections of districts of country.

"3. Practical Essays on subjects connected with civil engineering, such as geology, mineralogy, chemistry, physics, mechanic arts, statistics, agriculture, &c., together with models, drawings, or descriptions of any new and useful apparatus, or instruments applicable to the purposes of engineering or surveying."

The words in italics in Art. 4th have been so marked by us. We observed upon this liberal extension of the class of candidates soon after its first promulgation*, and when we felt it a duty to contrast it with an act of an opposite character, on the part of the Franklin Institute of the State of Pennsylvania, viz., that of indirectly narrowing the circle of those who might be benefited by a bequest of similar character entrusted to their care. We at that time appealed to the justice of the Franklin Institute for a revision and correction of their announcement of "Scott's Legacy Premium." It appears we did so in vain, for on the wrapper of the last number of their Journal (April), which has been received in England, the advertisement (and we fear it is *stereotyped*,) is still addressed to the ingenious of the United States, as the only "ingenious men and women who make useful inventions," and qualified to receive the legacy, thus confining to part—

> "what was meant for mankind."

A few extracts from an address of the present President of the Institution (Mr. J. Walker,) may be serviceable in exciting and directing such persons as may be in a position to make communications.

"The *will* to collect and to communicate is the only thing wanted. The materials are abundant—they are in the hands of almost every resident, who has the charge of a public work or public survey, whether river, canal, railroad, or public building, or who has the opportunity of seeing their working after they are finished. And if gentlemen in such situations, particularly young residents, would impress themselves with the feeling that they ought to communicate their observations, the benefit would be reflective, by leading to the habit of recording observations—a practice most useful for the obtaining of knowledge†.

"A person qualified to be a member or an associate of this Institution, cannot possibly visit a public work, or examine the survey of a river, canal, or railway, without seeing abundant materials in the hands of the resident engineer or surveyor, for useful practical communications. Every new fact, however *isolated*, is of great value to the Institution, one of the important objects of which is to collect and record facts. Observations of facts may be considered as experiments on a large scale; they ought, therefore, to be more correct than the latter, and will be so, if only carefully made. To make experiments on a large scale is often difficult, and so expensive that it can rarely be undertaken, solely for the sake of the experiments: but when the observations are made in the course of the profession, they are the cheapest in the making, and, moreover, the most valuable when obtained. Some, indeed, cannot be depended on, unless made at full size, particularly hydraulic experiments, where, to use the expression of one of our Honorary Members, 'we ought to have a model of the water,' to be correct in experimenting upon it with models: and there is the same objection to all experiments with models, wherever friction is concerned. How valuable, therefore, are observations carefully made in the most simple case, as a foundation for a theory! To note the velocities and resistance at different speeds of a boat in a canal, with a section and description of the canal and boat; to register the velocities of a river in different states, with the fall or slope of the surface, and the section in each case, noting also the differences of the velocity between different parts of the same section; to observe the actual discharge of sluices under different circumstances, and the work actually done by water; to record the construction, &c. of sea-walls and river and canal banks, and where a failure takes place to inquire into the cause of it."

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* Vol. I., pp. 339, 400.

† The importance of keeping a daily, nay, in some cases, even an hourly journal, cannot be too strongly impressed on the mind of every engineer; the amount of information which has been lost from the neglect of this practice, is incalculable.

Explosions of Steam-boilers.—Did Gas ever produce them?

THE recent disastrous explosion of the boiler of the Union steam-packet at Hull, was attended with numerous circumstances, which, when the excitement of the catastrophe has somewhat abated, may be the means of throwing an important light on this momentous question. They are scattered throughout the evidence, and we have no doubt but that their real value will be eventually accurately estimated on the spot, where it is evident there is ample ability for the most satisfactory inquiry.

We anticipate that the investigation will disembarrass the question of the conjecture, (for we believe it to be nothing more,)—that a sudden evolution of gas may take place in the boiler, and exhibit these terrific energies; this cause has, in all the instances we have examined, appeared to us to be quite superfluous. Steam has never wanted any fellow-labourer to aid it in the bursting of a boiler.

This conjecture has again made its appearance in the evidence at Hull. We hope, unbelievers as we are, that it may not be treated as chimerical; this course has frequently been tried, and the snake has, consequently, been scotched, not killed. We venture to suggest the following mode:—Let the gentlemen who have stated that, in their opinion, gas could be produced under such circumstances, and not only rival, but exceed steam in expansive force, be challenged, (not offensively, yet openly, and in a way that refusal may be difficult,) to produce what may be called an artificial explosion by this class of agents, generated from the materials and by the processes which have been followed by explosion in steam-boilers.

As it cannot be expected that private individuals can furnish the time and money necessary for such experiments, however well disposed, let a subscription be raised for the purpose, and handed over, under proper check, to the experimenters, to enable them to carry on the inquiry in the most satisfactory and complete manner, calling in to their aid, from any distance, such persons as they believe necessary and competent to assist them. The result, which we presume will be attained, whichever way it may be, must be a public benefit. It may possibly wound personal vanity in one quarter or other, but the amount of pain of this kind will depend upon the manner in which the inquiry is carried on; and even if it should prove to be very great, it ought not to have the slightest effect in obstructing or preventing an investigation so pregnant with the public good. The following evidence of Mr. Pearsall[*], given on the several inquests at Hull, we should think would stimulate some of the holders of the gas-hypothesis, to take every step that may be in their power, to demonstrate, if possible, the truth of the opinions they hold. The *onus probandi* certainly lies with them, and we hope they will go immediately to work, and that under the favourable circumstances we have suggested.

Mr. T. J. Pearsall stated—"I have considered the subject of the explosion of the boiler of the Union. In my opinion it could not have taken place had the boiler been sufficiently supplied with water, unless the steam generated was prevented escaping by some very great force, which should hold the safety-valve fastened down or obstructed. I consider the immediate cause of the bursting of the boiler to have been the expansive power of steam, because steam is capable of producing any such effects. I am most decidedly of opinion that it did not arise from gas. My reasons for that opinion are, that although water is decomposed readily by red-hot iron, yet it requires that the surface of the iron should be in a metallic state; a new boiler is nearly in that condition, but not quite so; there is just the chance that if a new boiler were employed, and very pure water in small quantity, so as to allow portions of the boiler to become red-hot, then water might become decomposed; but such circumstances would at the same time be sufficient to generate an incalculable amount of steam. Such explosion, therefore, would arise from mixed causes—such as the presence of the gas, and the enormous amount of steam and its great pressure. I cannot

[*] This gentleman was a pupil of Prof. Faraday,—was several years the assistant, in the Royal Institution, of that eminent investigator, and has recently retired from the Professorship of Chemistry in the Medical School at Hull, to direct the manufactures on a large scale, which eminently depend upon that science.

suppose a case where such decomposition of water and evolution of gas would take place, where such water is employed as that of the Humber, or as that furnished by the Ocean. Such gas would not be inflammable of itself,—there must be the presence of atmospheric air or oxygen, and of flame, or a substance heated to a very high degree.

"I can scarcely imagine such an explosion in a boiler, because there must be also a mixture of oxygen with hydrogen in proper proportion. I cannot conceive in what manner atmospheric air could gain admission to the boiler, the pressure of the gas from within being greater than that of the air from without, and the aperture which would admit the atmospheric air would allow the escape of the hydrogen. Hydrogen gas would operate upon the safety-valve in the same manner as steam. *I know not one single instance of one particle of evidence of the presence of hydrogen in a boiler.*

"When a boiler has been at work some time, and becomes incrusted with saline and other earthy matter, it is quite contrary to all known chemical principles, to suppose that water can be decomposed by such earthy substances; it is quite hypothetical to suppose that it can be so decomposed. I do not know that under any circumstances water could be resolved into its two elements—hydrogen and oxygen—by heat alone. In all cases the quantity of hydrogen generated must be very limited compared with the amount of steam formed by the high temperature. I would say that if hydrogen were in the boiler, and oxygen were to gain access, the hydrogen would escape by the same means as the oxygen entered; the pressure of all gases is the same when generated, and it is impossible to conceive that hydrogen should have less pressure than that of atmospheric air at the same temperature. It would require a very large portion of the oxygen of the atmosphere to be in contact with the hydrogen in the boiler to cause an explosion. In 100 parts of atmospheric air there are only about 20 of oxygen, and it is that alone which is required to explode the hydrogen, the nitrogen of the atmosphere having only the effect of diluting it. A possible introduction of air into a steam-boiler may be that of the air contained in the water itself, which may be evolved when the water is boiled, but this quantity of atmospheric air is very small, about four per cent. of the bulk of the fluid water, and not so much air as that can be obtained from sea-water. In round numbers, one cubic inch of water will afford 1700 cubic inches of steam, which, decomposed at that temperature, expands into thrice its volume of gases, two-thirds being hydrogen gas, requiring about 17,000 cubic inches of atmospheric air for full and rapid combustion, while the original cubic inch of water only gave off one-twenty-fifth of its bulk of air.

"If safety-valves be of the proper size, and steam be raised gradually, the valve will operate; but if you raise an enormous amount of steam at once, or under extraordinary circumstances, and wish it to be at once discharged, such circumstances as when you have too little water, and portions of the boiler be red-hot, and by a motion of the vessel any sudden evolution of steam is caused, then the valve will be rendered comparatively useless or dangerous. I consider steam capable of producing an explosion such as this. I know of no greater explosive power than steam, except gunpowder. Perkins's patent gun, in the Adelaide Gallery, daily throws off a number of balls per minute by steam; and if gas be produced by means similar to those alluded to, it must be produced in the instance of Perkins's gun; had the explosion been caused by gas, it must have been accompanied by a very vivid flash."

Materials for Mortars and Cements.

The study of the nature of different species of mortars, demonstrates that their solidification depends on the formation of silicate of lime, and sometimes also of silicate of alumine. These silicates retain some water, and assume the firmness of stone, whilst the hydrate of lime in excess unites by degrees with carbonic acid; and consequently, solidified mortar may be considered a compound of carbonate of lime and of a zeolite. *Opal, pumice stone, obsidian* and *pitch stone*, pulverized, form with hydrate of lime a good cement. But only the surface of each grain of *quartz* or *sand*, is transformed into a hydrated silicate; and though this is sufficient to unite the mass, solidification does not take place so promptly. The mass becomes the more solid, the more finely the quartz is pulverized. If the pulverized quartz be mingled with one-fourth part of lime, and after thoroughly calcining the mixture, it be pulverized and mixed with one-fifth part of lime, it forms a hydraulic cement which becomes so hard as to be susceptible of a polish. Feldspar hardens slowly, and with lime only after five months; but if calcined with a little lime it is much improved. Common potters' clay, which is absolutely worthless in its natural state, affords with lime, when calcined, provided it contains but little iron, a cement which readily hardens.

Steatite, which had been subjected to a red heat, cannot be made to unite with lime, and thence I concluded that magnesia has a strong affinity for silicic acid, attempted to employ calcined dolomite, in the place of ordinary lime, and that it surpassed it, both as a material for ordinary mortar, and also for a hydraulic cement. I obtained a good mortar with this last material and calcined marl.——FUCHS. *Berzelius' Jahrsbericht.*

Prize Subjects, 1837—*Zoological Society of London.*

THE subjects of Medals and Premiums offered in 1837 by the council of the Zoological Society of London, are as follows:—

1. For importing either a pair of Musk Oxen, or a specimen of the Hippopotamus, male or female; or a pair of the *Ornithorhynchus paradoxus*[*].

2. To the breeder of the greatest number of Curassows[†], between the Spring of 1836, and 1st of July, 1837.

3. To the importer of a male and female Indian Pheasant, of a species not already alive in this country.

4. To the breeder of the best specimens of Indian Fowls, bred between the Spring of 1836, and 1st of July, 1837.

5. To the breeder of the most rare or most interesting foreign quadruped, between the Spring of 1836, and 1st of July, 1837.

6. For the best essay on the care and treatment of the species of the genus *Felis*[‡] in confinement.

British Association.

THE next meeting of the British Association for the Advancement of Science is announced. It is to be held at Liverpool during the week commencing on Monday the 11th of September next. In the official advertisement of the above we find a sleepy note[§] which causes us to ask if any measures have been taken to prevent the disappointments, loss of time, and of temper, which took place during nearly one-half of the Bristol session. Some of these we pointed out soon after[||], and accompanied the notice of them with some suggestions, which we are sure ought to be deliberately examined by those who have the direction of the arrangements. Next to having good arrangements, is an early and extensive knowledge of their existence. Many of the parties who may wish to be present may live very far from Liverpool; perhaps, even from England. The periodicals which issue on the first day of July should convey all possible information. See what the French have done for a similar meeting, also (most unfortunately, and for want of the early and extensive knowledge we are advocating,) to be held in September[¶]. The *time* when they did it may be estimated by the following extract from Silliman's Journal, *published in the United States on the 1st of April last*, and recently arrived in this country.

"*Scientific Congress at Metz.*—A circular printed letter which we have received, signed by M. Victor Simon,

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[*] The Ornithorhyncus (*bird-beaked*, from *ορνιθος*, *bird*, *ῥυγχος*, beak,) Paradoxus obtained its name from Sir Everard Home. Shaw had previously named it Platypus Anatinus. It is a quadruped, which, instead of jaws and teeth, has a beak like a duck, and is therefore sometimes called the *duck-bill*; it is a native of New Holland: the settlers there have lately called it the *water-mole*. This most singular animal has not only the beak, but also the feet of a duck. Naturalists have not yet determined whether it be oviparous or not. Mr. Owen, about two years since, determined that it suckles its young, but its anatomical structure partakes more of the character of a bird than of a quadruped.

[†] Curassows are a genus of gallinaceous birds allied to the G. Phasianus, natives of *Chili and Peru*, something between a turkey *and a pheasant*, laying immense numbers of *eggs*, and being capital eating; there are *three or four* species in the Gardens.

[‡] This genus includes the lion, puma, tiger, panther, ounce, &c.

[§] Viz., "The members of the General Committee will assemble on the preceding Saturday," no *place* nor *hour* given.

[||] See Vol. II., pp. 136, 177, 254, 323, 327.

[¶] So is the Belgian Meeting!

Sécrétaire de l'Académie Royale de Metz, &c. &c. &c., announces that a scientific meeting of savans of France, Germany, and other countries, will be held at Metz, in September. It is desired that the purpose of attending may be made known, and also the subject which the individual will discuss. This is the fifth meeting of the French scientific men, held in different cities in the departments.

"Metz is about sixty-five[*] miles north-east of Paris; it is an ancient and famous city. It has eminent individuals, and various institutions for science and arts, for literature, humanity, and arms. It has fine Roman antiquities, and is in the midst of a beautiful country. We trust that some of our countrymen will attend, as they have done in England, as well as on the Continent."

Were "Messieurs les Sécrétaires" of the British Association thus early in the field? Are their missives operating at these distances?

Abstract of the American Additional Patents' Act.

THE Bill in the United States Legislature, which we recently noticed[†], has become a LAW: the progress of this Bill through Congress presents a singular contrast to that of a somewhat similar Bill, for the improvement of the Patent Laws, in our House of Commons; see our journal of it, p. 467. The Americans are in earnest; they move on with the "*quick step.*" Our manœuvre is that of "*marking time,*" an operation which consists in seeming to march, but without advancing a single step! Mr. Mackinnon appears an adept at this kind of *movement:* we regret it, for by it the bill is certainly lost for this session. At the general election, which is now expected, we trust Mr. Mackinnon will be asked for the history of his repeated and vexatious failures. Mr. Baines, his colleague for bringing in the Bill this session, has also incurred a responsibility[‡], which we hope the ingenious men of Leeds will require an account of at his hands.

In the next session, we advise such very inefficient friends will let this question alone, and that they will not, by intrusion into an important position, prevent the occupation of it by others who may resemble in zeal, intelligence, and exertion, the Hon. John Ruggles, of the Senate of the United States, to whom the American public are principally indebted for this important additional law.

The immediate object of this Act was the restoration of the records and models, &c., of the Patent Office, destroyed by fire on the 15th of December last, but the opportunity of improving the previous Act was not lost, and some important provisions have been inserted, particularly one affecting foreigners applying for patents in the United States. The following is a copious abstract.

"AN ACT
IN ADDITION TO THE ACT TO PROMOTE THE PROGRESS OF SCIENCE AND USEFUL ARTS."

Sect. 1.—Proprietors of patents issued prior to 15th December, 1836, may have them recorded, copied, and filed anew in the Patent Office, gratuitously.

Clerks of the judicial courts to transmit to Patent Office a statement and certified copies of all patents, specifications, &c., of inventions, &c., made before 15th of December, 1836, which may be on the files of their offices.

Sect. 2.—Copies of the registering, certified by the commissioner of the Patent Office, shall be prima facie evidence, same as originals, in any judicial court, without proof of the loss of originals.

No patent, nor any assignment of a patent issued prior to 15th of December, 1836, can be received in evidence after the 1st of June, 1837, unless it has been recorded anew, and verified copies of any drawings attached been deposited in Patent Office.

Sect. 3.—Patents lost on or before the 15th of December last, may, on application by parties interested, be replaced by a new patent, bearing date of original; *provided* a duplicate of the original, as near as may be, be deposited. Such new patents to be evidence to the same degree as the originals would have been.

Sect. 4.—Commissioners of Patent Office to obtain duplicates of such models destroyed as were valuable and inter esting, as may be important to the public, and necessary for commissioner for the discharge of his duties, and for the protection of the rights of patentees and the public; *provided* such duplicates be obtained at a reasonable expense, and that the total cost shall not exceed one hundred thousand dollars[*].

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[*] This is a great mistake; Metz being *one hundred and eighty miles* from Paris: it is nearly due east.

[†] Pp. 396, 397.

[‡] *See the Journal* of the Bill, p. 467.

Commissioner of Patent Office and two other persons appointed by President to form Temporary Board for determining what models shall be procured, and the most judicious mode of obtaining them. Such Board may make such conditions, &c., not inconsistent with law, as they may think proper.

Sect. 5.—Patentee on returning a patent for re-issue, and desiring separate patents for distinct parts, agreably to the Sect. 13 of former Act, to pay thirty dollars[†] for each additional patent, *provided* no patent prior to the 15th of December last shall be re-issued, nor any addition of an improvement to a previous patent, nor any new patent for an improvement in any machine, &c., be made, nor any disclaimer be recorded, nor any patent be granted for an invention, the models or drawings of which shall have been lost, until verified duplicates of the models and drawings of the thing, as originally invented, be deposited.

Sect. 6.—Patent may be issued to an assignee of the inventor on certain conditions being complied with.

Applicants for patents to furnish drawings in duplicate, one set to be deposited in Patent Office, the other annexed to the specification.

Sect. 7.—Patentee having by inadvertence, &c., claimed too much, may (in a patent in which some material and substantial part of the thing patented is truly his own,) disclaim the other parts. Disclaimer to be witnessed, recorded in Patent Office, and a fee paid of ten dollars[‡]. Such disclaimer to be considered part of original patent, but shall not affect any pending action.

Sect. 8.—All applications under this act to be subject to the same revision as original applications for patents; and applicants, if dissatisfied with decision of commissioner, have the same remedy as provided in the former law.

Sect. 9.—Over-claiming, without intention to defraud, not to vitiate the *rest of the* patent, *provided* the rest be *a substantial* part of the thing patented, and be "definitely distinguishable." Patentee may have his action for infringement of latter, but shall not be entitled to costs, unless he shall have entered at Patent Office a disclaimer of the over-claim prior to suit. Neglect, or delay to enter disclaimer to deprive plaintiff of benefit.

Sect. 10.—Commissioner may appoint agents in twenty principal towns to receive models, specimens, &c., intended to be deposited in Patent Office. Carriage of such to be paid from the patent fund.

Sect. 11.—Two examining clerks to be appointed instead of one, each to receive a salary of fifteen hundred dollars[*]; an additional copying clerk, salary eight hundred dollars[†]; temporary copying and draughting clerks, at discretion of commissioner; pay—not to exceed seven cents per page[‡] of one hundred words.

Sect. 12.—On application of a foreigner for a patent being rejected for want of novelty in the invention, he may receive back two-thirds of the duty he shall have paid.

Sect. 13.—Affirmation, in all cases when oath is required, may be substituted for it.

Sect. 14.—All money received for patents, fees for copies, &c., prior to the passage of the former Act, shall be carried to the credit of the Patent Fund. All salaries and expenses of the Patent Office to be paid out of this Fund. Commissioner to draw for the same, and to lay before Congress in every January a detailed statement of expenditure made by him, a classed list of all patents granted during the year, an alphabetical list of the patentees and their residence, a list of all patents which may have become public property during the same period, and "such other information of the state and condition of the Patent Office as may be useful to Congress or to the public."

Approved, March 3rd, 1837.

ANDREW JACKSON.

The "presidential assent" to this important Bill is said to have been the last official act of General Jackson; "he signed it the moment before his time of office expired." Would we could associate with the termination of the life and government of our late sovereign, the remembrance of such a boon to the Useful Arts! But *our friends* have been in the way!

[*] *£22,500.* [†] *£6. 15s.* [‡] £2. 5s.

[*] £337. 10s. [†] £180. [‡] About 4d.

A NOTA BENE *for the Ingenious of the United States.*

THE following case of a resident in the United States desirous of obtaining a patent in Europe, and perfectly destroying all chance of it by previous publication, is not the only one which has come under our observation. We wish to prevent such serious disappointments and the expense which attends the fruitless attempts.

In *Silliman's Journal* for April is a detailed and intelligible description of Davenport's Electro-magnetic Machine, drawn up from a personal inspection of it by Professor Silliman himself, and the words of the patent (American) given, stating in what this invention consists; and there is added, "arrangements have been made to take out the patent in Europe." They had better be immediately suspended, for in no European country would the patent, if obtained after this, be worth one farthing.

Saltness of the Red Sea.

AT a meeting of the *Royal Asiatic Society of Great Britain and Ireland,* in April last, the following paper by Mr. Malcolmson was read, giving the result of the examination of two bottles of sea water; one from the Indian Ocean, the other from the Red Sea.

"The officers of the *Hugh Lindsay* steamer having reported that, in consequence of the greater saltness of the Red Sea than of the Ocean, it was necessary 'to blow-off much more frequently while in that part of the voyage,' Lieut. Burnes had, therefore, brought to India a bottle of the water of the Arabian Sea, and another of the Red Sea, which were found to have (at the temperature of 86°), a specific gravity of 1·0254 and 1·0258 respectively, too small a difference to cause any sensible effect in blowing-off. But as lime might exist in greater quantity in the inland sea, the analysis was principally directed to ascertain the proportions of sulphuric acid and lime, the precipitation of which is the cause of the necessity of blowing-off.

"The following are the results:
an analysis by Dr. Ure, of a specimen

	Arabian Ocean.	Red Sea.
Sulphuric acid . .	1·82 . .	1·80
Lime	0·70 . .	0·82
Common Salt . .	32·8 . .	33·5

"Mr. Prinsep has quoted an account of water brought from near Berenice, by Mr. Wilkinson, to the following effect:—'The specific gravity is 1·035; and 1000 grains of water contain forty-three of saline matter, of which about four grains are muriate of lime, with a little muriate of magnesia, and the remainder muriate of soda, with a little sulphate of magnesia. The specific gravity of water of the open ocean, in the same latitude, is only 1·028, and contains not more than thirty-six grains of saline matter in a similar quantity.' Mr. Prinsep accounts for the discrepancy, by supposing the water examined by Dr. Ure, to have been obtained from an insulated position near the shore, and adds,—'the hydrometer is in all cases the safest test, and it is a pity that it had not been resorted to in the steam navigation of the Mediterranean sea, which has been the source of such contradictory statements.'

"Having furnished myself with the necessary apparatus, previous to my return from India by the Red Sea, the beginning of last year, with a view of this very subject, I found the specific gravity of the sea, from Mangalore in Malabar, along the Indian coast to Bombay; in that harbour (in December, 1835), and of the ocean between India and Cape Furtak and Bab-el-Mandeb, in Arabia, to be 1·0265. The temperature of the water did not vary more than a few degrees, and, being barely sufficient to cause a difference of a few hundredths of a grain, may be safely neglected. The temperature of the sea opposite to Mangalore was 83°, which it retained till north of Fort Victoria, latitude 17° 56′ North, when it was found to have fallen to 79°, and in Bombay Harbour was as low as 76°. Throughout the voyage to the Arabian coast (in January) it was about 79°, when the winds were slight, but was remarkably affected by the freshening of the breeze, falling soon after to 76°, that of the air varying from 74° to 76°. On the 12th of January, when coming in sight of Cape Furtak, the sea was 75°;—the thermometer in the cabin being 73° at eight, A.M., and 75° at three, P.M., the wind blowing pretty fresh from N.E. by N., and the wet-bulb thermometer falling to 65°·5; while Daniell's hygrometer failed to form any dew with ether of ordinary strength.

"In the roads of Mocha, and near Kamran Island, the Red Sea hardly differed in specific gravity from the ocean, while off Cosseir, at a considerable distance from the shore, it attained the specific gravity of 1·035, nearly corresponding to that brought from Berenice by Mr. Wilkinson. When it is considered that no river runs into this long and narrow gulf, surrounded by dry and burning sands, it is not to be wondered at, that it should be so remarkably salt; an effect hardly visible at Mocha, from its proximity to the ocean, from which a current almost constantly sets into the sea. I regret that severe sickness prevented my ascertaining the rate at which the density of the sea increases on sailing from Mocha to Cosseir. Had Mr. Burnes stated from what part of the sea he took the water, I have no doubt that the result of Mr. Prinsep's examination would have corresponded with that above given.

"That this great increase of specific gravity would sensibly affect the boilers of a steam-engine, will be evident from the following remarks:—Dr. Wollaston, in the Philosophical Transactions for 1829, in a paper on the saltness of the Mediterranean, gives a formula for finding the quantity of salt by the specific gravity, which I have found to give sufficiently correct results, when compared with the experiments on the waters of the Indian Seas. He directs the excess of the specific gravity to be multiplied by ·144, which gives the saline contents, plus (+) a quantity of water retained by the deliquescent salts when dried at 212°. In the present case, it will be better to employ the factor ·134, which will give the salts dried at a temperature at or above 300°. Hence, if a portion of water of the Indian Ocean[*], of specific gravity, 1·0265, which I found it, afforded sulphuric acid 1·82 grains, and lime ·70, that of Cosseir would, in the same bulk, furnish 2·40 grains, and ·955 of a grain; or about *one-third* more, which, in so insoluble a salt as sulphate of lime, must exert a considerable influence over the rapidity of deposit in the boilers, and be the cause of some delay on the voyage."—MALCOLMSON. *Royal Asiatic Soc. Jour.*

French Scientific Meeting.

THE fifth scientific meeting of the French Savans will be held in Metz in September.

Belgian Scientific Meeting.

THE second meeting of the National Belgian Association of Scientific and Learned Men, will be held in Ghent in September.

German Scientific Meeting.

AT the conclusion of the German Association of Naturalists at Jena last year, it was determined, that the one for 1837 should be held at Prague. It is generally in September.

Telegraphs, United States.

THE House of Representatives of the United States passed a resolve, February 3, 1837,

"That the Secretary of the Treasury be requested to report to the House of Representatives, at its next session, upon the propriety of establishing a system of telegraphs for the United States."

In compliance with this resolution, the Secretary, the Hon. Levi Woodbury, has issued a circular, with the view of obtaining information in regard to "the propriety of establishing a system of telegraphs for the United States," and invites the collectors, commanders of revenue-cutters, and others, to furnish the department of state with their opinions upon the subject, especially by pointing out the manner, and the various particulars, in which the system may be rendered most useful to the Government of the United States, and the public generally. It would be desirable, to present a detailed statement as to the proper points for the location, and distance of the stations from each other, with general rules for the regulation of the system, together with an opinion as to the propriety of connecting it with any existing department of the government, and some definite idea of the rapidity with which intelligence could, ordinarily, and also in urgent cases, be communicated between distant places. An estimate of the probable expense of establishing and supporting telegraphs, upon the most approved system, for any given distance, during any specified period, is also desired. Information and opinions are also asked as to the practicability of uniting with a system of telegraphs for communication in clear weather, and in the day time, another for communication in fogs, by cannon or otherwise, and in the night by the same mode, or by rockets and fires; and returns are asked by the 1st of October, 1837.

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[*] "I have used Mr. Prinsep's analysis and my own specific gravity of the Indian Ocean, as by this means the proportion between the saltness of the ocean and the *Sea of Cosseir* is better seen. If this is *objected to, it* will make the excess still *greater.*"

As the subject is one of high importance to national and individual welfare, especially in a country of such vast extent and diversity of interests and physical features as the United States, we trust that the call of the House of Representatives and of the Honourable Secretary will meet with prompt attention and full replies.—*Silliman's Jour.*

Proposed Standard Unit of Solar Radiation.

IN the instrument designed by Sir John Herschel for measuring the intensity of solar radiation, and which he named "the Actinometer;" the degrees have hitherto been arbitrary, and consequently, no two of the instruments were comparable. In a recent communication from the Cape of Good Hope, the inventor proposes to remedy this great objection to the universal use of the instrument in the following manner:—

"Let the term *actine* be used to express that intensity of solar radiation which would be competent to melt one millionth part of a metre in thickness of a surface of blackened ice, perpendicularly exposed, in one minute of time. Then each degree of the scale in question has been ascertained to be equivalent to 6·1 ($=6\frac{1}{10}$) actines. I will venture to hope, that the actine so defined, will henceforward be accepted as the standard unit of solar radiation." We have no doubt of the reasonableness of the hope of Sir John Herschel, but it certainly must depend upon the further explanations promised, before the "reason of the hope that is in him" can be effectually felt by others.

Extensive Meridian Line in Cornwall.

THE Committee of the Cornwall Polytechnic Society are on the eve of completing a meridian-line of considerable extent. The spire of St. Keverne Church is the terminus at the southern extremity of the line, and the other end rests, for the present, in a field to the westward of Beacon Hill, near Falmouth. A granite pillar is intended to be erected to indicate it. This will give a line of about 40,000 feet, (full 7½ miles,) which it is hoped will be continued through the county, to the British Channel on the north.

This accurate determination of the true meridian will furnish a standard, by which the variation of the magnetic needle may be readily ascertained, and its changes be easily detected. Compasses, &c., may be adjusted, and mine-surveyors, &c., be enabled to lay down their plans to the true meridian. [*Fourth Report Cornwall Polytech. Society.*]

The Three Believers.

OUR article "On an incredible experiment in which the human body loses its weight, related by Sir David Brewster and another[*]," has been copied, *literatim et verbatim*, into a Calcutta periodical, entitled "The India Review of Works of Science, &c., edited by Frederick Corbyn, Esq.;" and to that part of the article which describes the careful examination of the question by an "Experimental Society" in London, the Editor has appended the following note:—

"Notwithstanding the conclusions at which the Experimental Society have arrived, we recommend to our readers a trial of the experiment, and they will be satisfied that the effects are really astonishing.——ED. *India Review.*"

That the Editor of the India Review, a medical man, should, in the face of the investigations described, have contented himself with the mere assertion of so improbable a circumstance is to us astonishing. He has fallen into the error which it was the object of our article to warn eminent men from committing. And we think it a duty to repeat, "that scientific men should abstain from giving currency to such monstrous improbabilities, unaccompanied either by refutation or explanatory remark. It would be far better to continue the inquiry into the cause, either of the fact, or of the error, and abstain from publication until some satisfactory information had been obtained."

There are now, in three very distant parts of the world, three believers[†] in this apparent suspension of the laws of gravitation. The nature of the experiment, and the distant positions of the observers, require there should be, at least, twelve others. Perhaps also Major H., who, according to Sir David Brewster, performed it in his presence, and in that of the late Sir Walter Scott, and the officer of the American navy, who directed the experiment at Venice where Major H. saw it, may still be living. Let us hope that some of these will repeat the experiment by a process as unexceptionable, and before witnesses as competent and disinterested as those of the Experimental Society.

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[*] Vol. I., p. 104.

[†] Sir David Brewster, in Scotland.
Mr. Nickals . . . Upper Canada.
Dr. Corbyn . . . Calcutta.

The invitation we were empowered to make from the Experimental Society to Sir David Brewster, that he would direct them in the performance of another experiment, has produced no result, so far as the Society are aware.

Zodiacal Light.

I HAVE been somewhat disappointed, that astronomers should have paid so little attention to the remarkable changes which take place in the Zodiacal Light, about the 13th of November, as has been repeatedly mentioned in this Journal. It appears to me a fact deserving their attention, that the Zodiacal Light, which for weeks before the 13th of November appears in the morning sky, with a western elongation of from 60 to 90 degrees from the sun, (while up to that time not a glimpse of it can be caught in the evening sky,) should immediately afterwards appear after the evening twilight in the west, and rapidly rise through the constellations Capricornus and Aquarius, to an elongation of more than 90 degrees eastward of the sun, while it as rapidly withdraws itself from the morning sky, and within a few days vanishes entirely from the western side of the sun. For three years past I have observed these changes with much interest, and feel warranted in asserting, that they have been repeated with uniform regularity. The present year, the light was very feeble in the morning sky, an effect partly owing to the presence and peculiar splendour of the planet Venus; but as soon after the 13th of November as the absence of the moon would permit observations, the light appeared in the west *immediately after twilight*, crossing the Milky Way, and rising in a pyramid almost as bright as that, the triangular space between it and the Galaxy, embracing the Dolphin, appearing, by contrast, strikingly darker.

I can account for this great and rapid change of place in the Zodiacal Light, a change which is unlike any it sustains at any other period of the year, only by supposing, that on or about the 13th of November it comes very near to us, and that we pass rapidly by it, thus giving it a great parallactic motion, an effect which is in perfect accordance with all our previous conclusions.

According to this view of the subject, the Zodiacal Light would no longer be regarded as a portion of the sun's atmosphere, but as a nebulous or cometary body, revolving around the sun within the earth's orbit, nearly in the plane of the solar equator, approaching, at times, very near to the earth, and having a periodic time of either one year, or half a year, nearly.

Such, I affirm, would be the fact, should the Zodiacal Light be proved to be the body which affords the meteoric showers. —OLMSTED. *Silliman's Journal.*

Patent-Law Grievance. No. XVI.

THE inventors of this country, and the introducers of inventions of other countries into this, were obliged to pay down to the attorney-general and other agents, &c., of the Government, during the ten years ending December, 1834, more than £313,000, and during the past year above £42,000 (being at the rate of £420,000 in ten years.) It is to be observed that these enormous extortions are *exclusive* of the drawings, engrossings, and all the other charges of the patent-solicitor.

The penalties inflicted on the inventive genius of Britain during the *present* year, up to the 25th ult., in the shape of government stamps and fees on patents, amount to more than £23,000!

N.B. This sum has been paid in *ready money* on taking the first steps, and as many of the inventors are poor men, (*Operatives,*) and a great many others of them persons to whom it would be very inconvenient to pay at least £100 down, they have been obliged to go into debt, or mortgage or dispose of their inventions, either wholly or in part, &c.

NEW PATENTS. 1837.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second, that on or before which the Specification must be delivered and enrolled.——The abbreviation *For. Comm.*, signifies that the invention &c., is "a communication from a foreigner residing abroad."

May *contd.*

112. James Partridge Blake, Little Queen-st., St. Giles's, *Middx.*, Engineer; for improvements in apparatus for hulling, dressing, &c., paddy or rough rice, oats, and such other grain, part of which is applicable to other purposes. May 30.—Nov. 30. *For. Comm.*

113. Joseph Woollams, Wells, *Som.*, Gent.; for improved means of obtaining power and motion from known sources. May 30.—Nov. 30.

114. Francis William Gerish, East-rd., City-rd., *Middx.*, Smith and Ironmonger; for improvements in the apparatus for closing doors, gates, and shutters. May 30.—Nov. 30.

Total, May...26.

June.

115. Richard Oke Millett, Peupalls Hayle, *Cornw.*, Gent.; for improvements in instruments for extracting teeth. June 1.—Dec. 1.

116. Edward Schmidt Swaine, Leeds, *York.*; for a method of producing and preserving artificial mineral waters, and for machinery to effect the same. *Term extended seven years, from 9 Oct. next.*

117. Joseph Clisild Daniell, Limpley Stoke, *Wilts.*, Gent.; for improvements applicable to stone-masonry. June 6.—Dec. 6.

118. Miles Berry, Chancery-lane, Holborn, *Middx.*, Mechanical Draftsman; for improvements in obtaining motive power for propelling or working machinery. June 6.—Dec. 6. *For. Comm.*

119. John Kirkham, Aldenham-terrace, St. Pancras-rd. *Middx.*, Engineer; for an improved mode of removing the carbonaceous incrustation from the internal surfaces of retorts employed in the process of distilling coal for generating gas. June 8. —Dec. 8.

120. John George Bodmer, Bolton le Moors, *Lanc.*, Civil Engineer; for improvements in machinery for spinning and doubling cotton, wool, silk, flax, and other fibrous materials. June 12.—Dec. 12.

121. Godfrey Woone, Berkeley-st., Piccadilly, *Middx.*, Gent.; for an improved method of forming plates with raised surfaces thereon, for printing impressions on different substances. June 12.—Dec. 12.

122. William Fothergill Cooke, Breeds-place, Hastings, *Sussex*, Esq.; and Charles Wheatstone, Conduit-st., Hanover-sq., *Middx.*, Esq.; for improvements in giving signals and sounding alarums at distant places by means of electric currents transmitted through metallic circuits. June 12.—Dec. 12.

123. Richard Roe, Everton, near Bawtry, *York.*, Gent.; for improvements in machinery or apparatus for making bricks, tiles, and other articles made from earthy materials. June 17.—Dec. 17.

124. James Leonard Clement Thomas, Covent Garden, *Middx.*, Esq.; for an improvement applicable to steam-engines and steam-generators, having for its object economy of fuel. June 17.—Dec. 17. *For. Comm.*

125. William Nicholson, Manchester, *Lanc.*, Engineer; for improvements in the construction and arrangement of preparation and spinning machinery. June 17.— Dec. 17. *For. Comm.*

126. James Buckingham, Great Randolph-st., Camden Town, *Middx.*, Civil Engineer; for improved combinations of machinery to be applied as mechanical agents in a great variety of situations, in which toothed gear and other mechanism have been heretofore employed. June 17. —Dec. 17.

127. Theophilus John Nash, John-st., Downshire Hill, Hampstead, *Middx.*, Letter Maker; and John Ross, Wild-st., Lincoln's-Inn Fields, *Middx.*, Brass Worker; for a method of manufacturing in metals, wood, and other substances, and materials, letters, figures, and other devices, having a flat surface, presenting by the aid of colours the appearance of projection and domed letters, figures, and other devices made from the same materials without seam or joint. June 19.—Dec. 19.

128. William Yetts, Yarmouth, *Norf.*, Merchant; for an improved mode of caulking ships and other vessels. June 19.— Dec. 19.

METEOROLOGICAL JOURNAL FOR MAY, 1837; KEPT AT BLACKHEATH ROAD.

Day of Month		Barom. 9 A.M.	Ther. attch.	Barom. 3 P.M.	Ther. attch.	Thermometer. Min.	Thermometer. Max.	Daily Temp.	Solar Var.	Rad.	Clouds. A.M.	Clouds. P.M.	Winds A.M.	Winds P.M.	Direction of Wind A.M.	Direction of Wind P.M.	Lunation.	WEATHER, &c.
Mon.	1	29·762	59°	29·800	60°	46°0	60°6	53°3	14°6	44°	5	3	3	3	swbw	wsw		Wind very high; cloudy. *First notes of cuckoo heard.*
Tues.	2	30·018	60	29·982	61	44·7	62·5	53·6	17·8	42	5	6	2	1	W	W		Very fine; *cumuli. Nightingale sings.*
Wed.	3	29·880	62	29·755	61	45·2	63·0	54·1	17·8	43	7	7	2	2	E	NE		Generally cloudy and mild.
Thurs.	4	29·882	59	29·948	61	42·9	56·8	49·9	13·9	43	10	5	2	1	N	Nly	●	Cold and cloudy, A.M.; Afternoon fine.
Friday,	5	30·100	58	30·103	60	39·4	56·5	48·0	17·1	37	2	3	2	1	NW	WNW.N		Fair; *cumuli;* cold at Night.
Satur.	6	30·201	58	30·192	59	34·0	55·6	44·8	21·6	31	3	5	1	1	NbE	NE		Hoar frost; fine but cold; *cumuli.*
SUN.	7	30·118	57	30·043	58	33·8	55·8	44·7	22·0	30½	5	5	0	0	NE	wly		Hazy, gloomy, and cold.
Mon.	8	29·882	57	29·782	57	40·8	55·6	48·2	14·8	40	10	7	1	0	swly	WSW		Overcast, with showers; rainy Night.
Tues.	9	29·682	56	29·662	56	36·9	47·7	42·3	10·8	37	7	5	1	1	N	NE. N		Remarkably cold; *cumuli* and *cum.-stratus;* clear at Night.
Wed.	10	29·681	54	29·749	54	31·0	46·5	38·7	15·5	27½	5	7	2	2	N	N		Hoar frost, with *ice; sleet* and *snow* fell in showers.
Thurs.	11	30·032	53	30·050	53	33·9	51·5	42·7	17·6	31	5	7	1	0	N	N. E		Do. P.M., more moderate; cloudy Night.
Friday,	12	29·900	53	29·835	55	39.8	57·2	48·5	17·4	36½	10	4	1	1	SE	W	☽	Rainy; Afternoon and Evening fine.
Satur.	13	29·901	56	29·846	57	36·6	59·0	47·8	22·4	36	5	5	1	1	WSW	SW		*Nimbi,* with showers.
SUN.	14	29·844	57	29·855	59	37·8	57·0	47·4	19·2	35	5	7	0	2	NE	N,		*Stratus* and mist, A.M.; thunder-storm to N.; cold and cloudy Evening.
Mon.	15	30·152	58	30·147	58	42·9	56·4	49·6	13·5	42	8	7	2	2	N	N		Cloudy, P.M.; cold, with small rain.
Tues.	16	30·352	58	30·358	58	42·9	56·8	49·8	13·9	41	9	1	2	2	NbE	NbE		Cloudy till Noon; fine, P.M.; *Parhelion* at 6 P.M.
Wed.	17	30.344	59	30·249	62	38·0	67·9	52·9	29·9	35	2	7	1	2	W	NW		Fine Morning, and warm; Evening, sharp cold wind.
Thurs.	18	30·248	61	30·200	62	46·6	53·0	49·8	6·4	45	5	4	2	3·2	NbE	NNE		Wind, bleak and sharp; *cumuli.*
Friday,	19	30·130	58	30·108	58	39·7	53·6	46·6	13·9	38	7	0	2	1	NE	NE. N		Rain mixed with *sleet;* Evening cloudless, but hazy.
Satur.	20	30·038	57	29·955	57	36·8	49·8	43·3	13·0	34	9	7	2	2·1	NbE	NNE	○	Much overcast; *cirro-stratus.*
SUN.	21	29·780	55	29·762	55	39·9	49·0	44·4	9.1	38½	10	10	1	1	WNW	N		Small rain; cloudy.
Mon.	22	29·806	54	29·851	55	37·0	51·3	44·1	14·3	33	7	5	2	3·1	Nbw	NbE		Stormy, with showers; Evening partly cloudy.
Tues.	23	30·005	54	30·032	56	38·5	54·8	46·6	16·3	34	8	5	2	1	N	NE		Cloudy, A.M.; fine Afternoon.
Wed.	24	30·001	56	29·962	58	33·0	61·7	47·3	28·7	31	7	8	0	3	W	SW		Fine day, but much overcast.
Thurs.	25	29·882	57	29·875	58	35·1	62·2	48·7	27·1	32	5	5	0	1	S	SW		Fine; a haze in the *upper* atmosphere.
Friday,	26	29·948	60	29·940	63	45·2	67·1	56·2	21·9	43	5	5	1	1	SSW	SSW	☾	*Cumulus;* air soft; fine.
Satur.	27	30·050	61	30·056	63	37·0	67.0	52·0	30.0	34	2	2	1	1	wly	SW		Very fine.
SUN.	28	30·103	62	30·045	63	41·9	59·5	50·7	17·6	40	7	10	2	1	E	E		Cloudy.
Mon.	29	30·050	62	30·048	64	50·0	66·6	58·3	16·6	49	3	1	2	2	WSW	WSW		Fine; *cumuli;* cloudy Evening.
Tues.	30	30·128	63	30·100	63	45.1	63·5	54·3	18·4	44	5	3	3	3.2	W.	W		Strong wind; *nimbi;* mostly clear.
Wed.	31	30·091	63	30·002	64	44·0	64·8	54·4	20·8	42	10	7	2	2	SW	W		Light rain; generally cloudy.
Mean		29·999	58	29·977	59	39·87	57·75	48·81	17·88									

Bar. Max. 30·395 in. on the 16th.
Bar. Min. 29·662 in. „ 9th.

Ther. Max. 67·9° on the 17th.
Ther. Min. 31·0° „ 10th.

Lowest point of Rad. 27½°, on the 10th.
Rain fallen (by Crossley's self-registering gauge), 1·220 in.

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